

FIELDIANA

Anthropology

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VOLUME 65

CHAPTERS IN THE PREHISTORY OF EASTERN ARIZONA, IV

PAUL S. MARTIN

EZRA B. W. ZUBROW

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February 17, 1975

FIELDIANA: ANTHROPOLOGY

A Continuation of the

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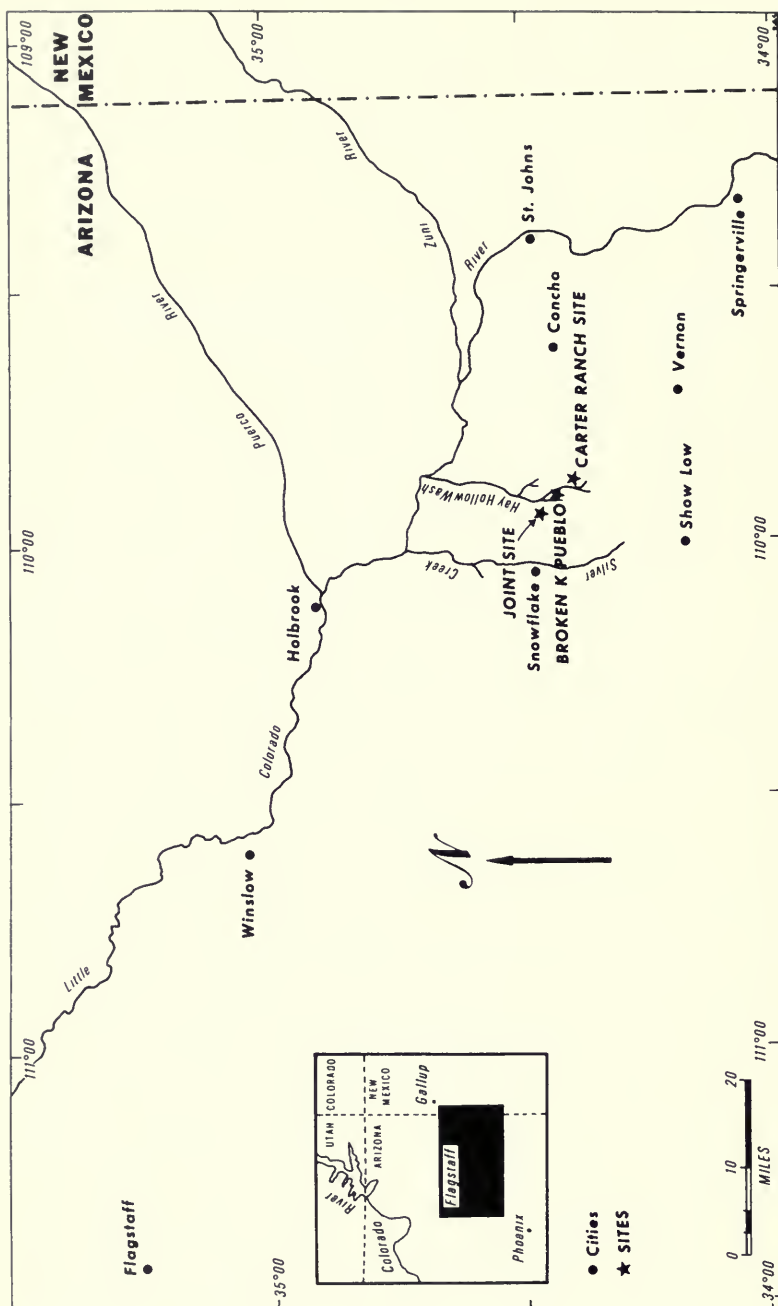
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VOLUME 65



FIELD MUSEUM OF NATURAL HISTORY
CHICAGO, U.S.A.

CHAPTERS IN THE PREHISTORY
OF EASTERN ARIZONA, IV



FRONTISPIECE. Map of study area in east-central Arizona.

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PAUL S. MARTIN

*Late Chairman Emeritus, Anthropology
Field Museum of Natural History*

EZRA B. W. ZUBROW

*Department of Anthropology, Stanford University
Stanford, Calif.*

DANIEL C. BOWMAN

*Department of Sociology and Anthropology, Hamline University
St. Paul, Minn.*

DAVID A. GREGORY

*Department of Sociology-Anthropology
New England College
Henniker, N. H.*

JOHN A. HANSON

*Department of Anthropology, University of Arizona
Tucson, Ariz.*

MICHAEL B. SCHIFFER

*Program in Anthropology, Prescott College
Prescott, Ariz.*

DAVID R. WILCOX

*University of Arkansas Museum
Fayetteville, Ark.*

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This volume is dedicated to

Dr. Paul S. Martin
(1899-1974)

He was both our teacher and our
colleague, but more importantly,
he was our friend.

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Contents

	PAGE
LIST OF ILLUSTRATIONS	xi
I. INTRODUCTION	1
<i>Paul S. Martin</i>	
II. PHILOSOPHY OF EDUCATION AT VERNON FIELD STATION	3
<i>Paul S. Martin</i>	
III. PRELIMINARY COMMENTS ON THE ALLUVIAL CHRONOLOGY OF THE HAY HOLLOW VALLEY, EAST-CENTRAL ARIZONA	12
<i>Daniel C. Bowman</i>	
IV. ECOLOGICAL PERSPECTIVES IN THE HAY HOLLOW VALLEY	17
<i>Ezra B. W. Zubrow</i>	
V. DEFINING VARIABILITY IN PREHISTORIC SETTLEMENT MORPHOLOGY	40
<i>David A. Gregory</i>	
VI. THE JOINT SITE—A PRELIMINARY REPORT	47
<i>John A. Hanson and Michael B. Schiffer</i>	
VII. STRESS RESPONSE IN CULTURAL SYSTEMS: A PREHISTORIC EXAMPLE FROM EAST-CENTRAL ARIZONA	92
<i>John A. Hanson</i>	
VIII. BEHAVIORAL CHAIN ANALYSIS: ACTIVITIES, ORGANIZATION, AND THE USE OF SPACE	103
<i>Michael B. Schiffer</i>	
IX. A STRATEGY FOR PERCEIVING SOCIAL GROUPS IN PUEBLOAN SITES	120
<i>David R. Wilcox</i>	
REFERENCES	166

List of Illustrations

Map of study area in east-central Arizona	Frontispiece
---	--------------

TEXT FIGURES

	PAGE
1. Preliminary classification of microhabitats in the Hay Hollow Valley	19
2. Map of Hay Hollow Valley showing microhabitats and ecological survey sample units	21
3. Transect and nested quadrat configurations	25
4. Floor of single room site, N.S. 702.	43
5. Map of Joint Site Pueblo (N.S. 605), East Central Arizona	50
6. Detail map of central room block of Joint Site Pueblo	51
7. Joint Site, northwest section with wall outlines exposed	54
8. Joint Site: Room 2, west wall.	60
9. Joint Site: Room 7, east wall	61
10. Joint Site: Room 8, floor.	62
11. Joint Site: Room 9, floor.	63
12. Joint Site: Room 10, east wall.	65
13. Joint Site: Room 12; floor 1.	66
14. Joint Site: Room 12, floor 2.	67
15. Joint Site: Room 15, floor.	68
16. Joint Site: Room 15, "Pilaster"	69
17. Joint Site: Room 23, floor.	70
18. Joint Site: Room 24, floor.	71
19. Joint Site: Room 29, floor.	73
20. Joint Site: Room 31, floor.	73
21. Joint Site: Room 34 (Kiva) floor (with firepits).	74
22. Joint Site: Room 34 (Kiva), west wall.	75
23. Joint Site: Room 34 (Kiva), east wall.	75
24. Average density of lithic and ceramic counts for each provenience unit	83
25. Joint Site: Test square S7W6, north face.	85
26. A hierarchical taxonomy of food preparation activities of the Hopi, ca. 1900	107
27. Joint Site: Room 2, facing south.	140
28. Joint Site: Room 5, facing south.	141

29. Map of Joint Site Pueblo, showing wall relations, wall openings, and building sequence	142
30. Joint Site: Room 1, facing west.	143
31. Joint Site: Room 17 and 20, facing north.	145
32. Joint Site: Room 11, facing south	146
33. Snowflake Black-on-White, Carterville variety, pitcher; Joint Site Pueblo, found with burial number 1.	160
34. Snowflake Black-on-White, Snowflake variety, bowl; Joint Site Pueblo, found with burial number 3.	160
35. Snowflake Black-on-White, Snowflake variety, pitcher; Joint Site Pueblo, found with burial number 3.	161
36. Snowflake Black-on-White, Snowflake variety, bowl; Joint Site Pueblo, found with burial number 3.	161
37. Snowflake Black-on-White, Snowflake variety, bowl; Joint Site Pueblo, found with burial number 6.	162
38. Show Low Black-on-Red, bowl; Joint Site Pueblo, found with burial number 7.	162
39. St. John's Polychrome, bowl; Joint Site Pueblo, found with burial number 9.	163
40. St. John's Polychrome, bowl; Joint Site Pueblo, found with burial number 9.	163
41. Snowflake, Black-on-White, Snowflake variety, bowl; Joint Site Pueblo, found with burial number 13.	164
42. Snowflake, Black-on-White, Snowflake variety, jar; Joint Site Pueblo, found with burial number 14.	164
43. Snowflake Black-on-White, Snowflake variety, duck effigy; Joint Site Pueblo, found on floor, room 7.	165

TABLES

1. A tentative alluvial sequence of Hay Hollow Valley	14
2. Correlation coefficients, r and ρ , of the total numbers of plants by species by microhabitats	27
3. Correlation coefficients, r , of numbers of plants by species by microhabitats using mean data	28
4. Correlation coefficients, r and ρ , of animal densities by microhabitat	29
5. Total plant distribution for all quadrats by microhabitat	32
6. Total animal transect data	36
7. Total animal transect data by density per square mile	38
8. Burial information chart	88
9. Partial behavioral chain of maize for the Hopi, circa A.D. 1900	108
10. Tabulation of room floor areas, and wall and roof openings, Joint Site Pueblo	149
11. List of tree-ring dates from the Joint Site and their proveniences	151

I.

Introduction

Excavations were carried out at the Joint Site, about 10 miles east of Snowflake, Arizona. The ruin is on the ranch of Mr. and Mrs. James Carter, whose friendship, interest, and help in our work are greatly appreciated. The site is located near Hay Hollow Wash at lat. 34°, 33' N and long. 109°, 57' W, G. and S. R. Meridian. The elevation is about 5,750 \pm 25 ft. above sea level.

The research was made possible by grants from Field Museum and the National Science Foundation (Undergraduate Research Participation Program grants GY-5973 [1969], GY-7225 [1970], and GY-8938 [1971]; and Research grants GS-2381 [1969-1970] and GS-27566 [1971-1972]). Mrs. Roderick S. Webster provided funds which were used for the preparation of this publication. I am most grateful for these financial assistances.

The work reported herein was conducted between July 1 and September 20, 1970 and 1971. The work was under the joint direction of Fred Gorman, David Gregory, John Hanson, and Michael Schiffer. They were assisted by student participants—for 1970: David Burkenroad, Michael Ester, Eric Gritzmacher, John Johnson, Colleen Maley, Sharon Ott, Paul Parker, Margaret Powers, John Rick, Stephen Saraydar, Jerome Schaefer, Susan Tracz; and for 1971: Susan Anderson, Dan Andrews, Russell Barber, Scott Cox, Alan Engstrom, Mark Henderson, Elsa Hirvela, Stanley James, John Justeson, Sue Tracz, Carole Wiley, and Hanson Wong.

We owe thanks to Bill Sampson and Dr. and Mrs. Gene G. Goode for help in excavation in 1970, and to James Moore for help in excavation and to Paul L. Smith, photographer and executive assistant in 1971. Douglas B. Cargo drew the map which appears as the frontispiece.

In addition to the report on the site, the analyses of which are not yet completed, Dr. Ezra B. W. Zubrow reports herein on his ecological researches in the valley (1970-1971), Daniel C. Bowman makes a preliminary report on the alluvial chronology of the Valley, and David

Gregory discusses the significance of single room sites in terms of pre-historic settlement morphology.

My contribution was to give a brief resume of our educational philosophy over the past 40 years and to document the drastic changes that have taken place.

Several doctoral dissertations will result from these various projects, and thus more formal and finished (although not necessarily complete) chapters will flow from these temporary essays.

PAUL S. MARTIN

LATE CHAIRMAN EMERITUS

DEPARTMENT OF ANTHROPOLOGY

FIELD MUSEUM OF NATURAL HISTORY

April 1, 1972

II

Philosophy of Education at Vernon Field Station

by

PAUL S. MARTIN

For over 40 years I have been “teaching” high school, college, and graduate students. My efforts have been confined to archaeological research carried on in the summer months in the American Southwest—Colorado, New Mexico, and now in Arizona. During this time, my philosophy about teaching has shifted several times and, although I am here primarily concerned with teaching as now practiced in our Undergraduate Research Participation Program, supported by N.S.F.,¹ I should like to dwell briefly on the “before” and the “after” in order to make clearer the contrasts of what I used to do and think, and what I now consider important.

1927 - 1966

My earlier concepts of teaching were most naturally reflections of the education I had received—an education almost unbelievably different from my present conceptions. In fact, I shrink from speaking of my training as “education” except in the narrowest sense. My eight years in grammar schools (partly in a very out-of-date Chicago school where our ears were yanked or our hands whacked with a ruler if we failed to give the “correct” answer or if we were too frightened to speak up; in a rural school in Nevada; and in a “modern” school in Winnetka, Illinois) were desolate, unhappy, and ruinous.

High school was better because there I had excellent teachers. Nonetheless, the emphasis during all those years was entirely on learning by rote, learning the RIGHT ANSWER (there was only one answer in those days), absorbing much misinformation and trivia, all of which was regurgitated at exam time. We were not encouraged or helped in the process of discovering facts and knowledge by our own thought processes. It was assumed that once the required classes in English, History, Science, and Math were passed, one need not worry about them again—as if we had

¹ National Science Foundation.

been immunized for life. This method of education was not unique to my generation or even the generation that followed. Small wonder then, in today's world of mass media, that a "credibility gap" between teachers and students currently exists.

Within the last 25 to 50 years much has happened. We are living in an environment that requires new adaptive strategies in order to survive. This new environment includes such things as television, movies, autos, long-playing records, electronic tapes, radio, telephones, electric lights, electric refrigerators, air conditioning. Such innovations have rendered obsolete much of what we had learned. Too few could re-adapt with ease if these appliances of modern life were removed. Think, then, how totally new is the environment of people 20 years old or less as compared with that of 30 to 50 years ago. New technological advances require a new kind of education—one that changes daily, monthly, yearly. (Postman and Weingartner, 1969). I sometimes feel as if I were a fountainhead of outdated information. Everything has changed—even change itself. The education that people over 35 years of age received actually retarded their development and made it all but impossible for them to develop new survival strategies or to accept new ideas.

What has this polemic to do with the students I have guided and "taught"? Everything. What I gave to those students was discolored by my limited background and education. Students were taught the rudiments of archaeological excavations—maintaining a perpendicular trench wall; the removal of burials; how to detect pit house walls; and how to recognize the floor of a room. I taught the taxonomy of pottery types and of stone tools. My goals, like those of other archaeologists at the time, were to reconstruct the history of the site and to make limited comparisons with other sites in the vicinity. At the research station we "filled in" time gaps; we searched for the "earliest" dates for houses, pottery types or choppers of stone; and when caves were found, the earliest corn. Archaeology was conjectural history and no more than that. The student was not a participant; he was merely a cog. He dug where and how he was told. Questions were discouraged or answered by the response, "because we have always done it that way." Inquiries were arrested by various devices. The favorite one was to give a name to an object, feature, type, or technique, thereby assuming you would know how it works and no further thought would be needed. "Name it and you know it" was the motto. Now, it is obvious that naming an idea neither explains a process nor how it works. Labels may be necessary but they are dangerous. If one calls a particular type of pottery "St. Johns Polychrome" that does not in any way indicate that this particular pottery type may have had certain

antecedents, may have been used only by certain members of the group, employed only at certain festivals, utilized for particular foods, or possibly devoted only to mortuary practices.

It does not direct attention toward such things as an inquiry of the relationships between technologies and decision-making processes. Naming something explains nothing; the label is not the process.

Students were not encouraged to use their senses, their intelligence, to ask "how come" or to conceive of problems. Yes, archaeologists were often puzzled as to why a kiva ventilator fronted north instead of south; would wonder "why," but never attempted to formulate an hypothesis about the matter. We would spend hours wondering about the presence or absence of kivas, without ever thinking of these facts as problems. Such trivia occupied our off hours but we rarely investigated the structure and relationships of the components of a site. We never dreamed of asking "how" you go about finding possible answers to such questions. In fact, the questions we were concerned with were possibly not worth wasting time on as, in retrospect, they seem to have been unimportant or unanswerable.

In short, we were not interested in the learning processes. We stressed conformity, "true answers" and did not encourage interaction between students or between students and staff, except on the most elementary levels. We did not think in terms of problems, deduction-induction, or the hypothetical mode of teaching. We operated within a closed-system where the answers were fixed. It never occurred to us that the students were capable of independent thought; we never granted them freedom from the intellectual and social constraints of our "closed" educational system. Our philosophy was really a threat to survival, was contributing to a rapid entropy and did not envision that our teaching should expose students to all aspects of a *total* field experience. We, the leaders, were urging dogmatism, intellectual diffidence, and fear of change. We were demanding of our students what our cramped, antediluvian school environments had demanded of us.

1966 - TO THE PRESENT

Now, things are different, and I hope, better. At Vernon our goals leave much to be desired but at least we seek to function with what Wiener (in Postman and Weingartner, 1969, pp. 3, 4) called "anti-entropic feedback systems." He insisted that we must have adequate feedback and instruments to indicate when standards are not being met by producing teachers who have been trained to recognize change, to be aware

of problems caused by change, and who have the courage to signal when we start to lose steam and to "run down." If a student attends our archaeological research station for ten weeks and leaves it "seeing" things exactly as he did when he started, then we on the staff consider that we have failed in our task because the student has learned nothing. We try to stress that learning is a happening that is brought about by inquiry.

The changes that have taken place and the changes that we introduce each year at our field station at Vernon, Arizona are primarily due to two phenomena: (1) a radical change in my own philosophy (Martin, 1971); and (2) a young staff composed of innovative, imaginative graduate students who are trying to teach our students to be aware of pitfalls, fraud, misrepresentation—to see beyond words to the ideology and its meaning; and mainly, to be braced for future shock by "telling it like it is"; that is, to give the students an education that will show them what the world really is—now—today.

To accomplish this, we have to teach them strategies for surviving in a world of unbelievably fast changes. Fifty years ago, the world changed slowly and consequently education was geared to very slow changes. Nowadays, change occurs so fast that even young people are often baffled because they have not been taught how to "roll with the punch" or to originate new techniques of survival.

But, to go back a bit - permit me to answer the question "How is your teaching different and why?"

In 1965, at the suggestion of the Director of Field Museum, Mr. E. Leland Webber, I decided to apply to National Science Foundation for a grant from their program called Undergraduate Research Participation Program. The proposal was favorably received and the appropriation made. At first, in order to use the limited equipment on hand, the program accepted only eight male students. (Later, the camp was enlarged and four women and eight men were accepted.)

I now blush at our naivete and lack of experience in handling what seemed to us a stable of high-spirited race horses. These students were so smart and so much ahead of all of us, even my youngest staff members, that we felt more out-dated than a Rip Van Winkle.

Selection was as follows: a poster announcing the program, "New Perspectives," was mailed to all departments of Anthropology and Sociology in the United States, and interested students were invited to write for a description of the program, application and reference blanks, and to send a transcript of their academic record. Sophomores and juniors interested in science were our preferred choices.

Thirty-three applications were received, 17 of which, for one reason or another, were disqualified. Eight males were chosen from the eligible applicants. Of those eight, six have gone on in anthropology, and are to receive their doctoral degrees.

Since that first program, there have been 80 student-participants, 70 of whom have gone on in graduate studies. As a result of our researches of seven seasons, eight doctoral dissertations and nine masters' theses have been produced.

I am proud of this record as perhaps it shows that students have left our research station acting and talking differently because they perceive things differently than they did before taking our course.

However, in spite of our wish to change and to give independence to the students, we wrote as follows in our first report to the N.S.F. Undergraduate Research Participation Program (1965). "After a basic theoretical orientation had been given to the students, the research projects were introduced, discussed at length, and the list posted. Each student chose one to his liking."

In our second year's (1966) report we made a more abject statement: ". . . . Although the participants have the opportunity of selecting their projects from a list of topics that is important to us (sic), they are not now permitted complete freedom to devise their own researches. Such freedom was previously found unworkable, in that too many of the students selected unfeasible or unimportant projects and were later dissatisfied with them." Think of talking about independence and then choosing the students' subjects for them! How can a bird learn to fly, if its wings are clipped? How can we teach independence and encourage sensitivities to develop and perceptions to be built except by letting the students generate their own problems, make mistakes and thereby learn by doing?

Needless to state, the people in charge at N.S.F. did not like what we were doing because they wanted the student free to choose his own project with as little guidance and direction as possible: "In the ideal situation, the student will be immersed in research and independent study in an area of particular faculty competence and interest. It is understood that under these conditions, the student will seek guidance, but care must be taken to assure that his role does not become an unduly dependent one. In no case is he to be an hourly laborer or a data collector whose experience does not include the fitting of theory to data" (*Guidebook for Directors*, U.R.P.P., N.S.F.; S.P.E. 64-F-5; page 2).

So, we changed again! This time I think our orientation was more in line with the inquiry method, because this is an art and a science in itself, and in line with dealing with problems that are perceived as “useful and realistic by the learners” (Postman and Weingartner, 1969, p. 81). The principal emphases are now on how to make meanings take root, live, and to develop; to learn what the scientific method is and how to use it; and whenever possible to develop hypotheses that will provide explanation of contemporary problems.

In the past few years, we have taken a hard line towards using the scientific method—in fact, we have insisted that it be used.

Now, by all the canons of good taste and good teaching, this is not going to bring about the results we want. In stressing the logic of science we have created a situation that some students did not want to become involved in. We either have to show them the advantages of the scientific method and persuade them that it is a useful tool employed by most scientists for proceeding from the general to the specific, from a known principal to an unknown, and is most useful for deriving law-like statements; or, we must let them muddle along in some unstructured and non-logical way of arriving at valid statements.

This is not the place to argue for or against the scientific method. In my own mind, it is the most interesting and quickest way of demonstrating cause and effect, of making a statement that will generally hold true because it is not based merely on a particular fact or individual case. The logic of all this appeals to me and I have found it most useful in solving problems in which I am interested. But it may not be best for everyone.

I would still urge the student who has never tried it, or who has had no training in science, to struggle through one problem with its aid in order to see if it has advantages over other known methods of dealing with hypotheses. But, I think that our insistence on having the student use only the hypothetico-deductive method has created unnecessary resistance and negativistic, hostile feelings that might not have arisen if we had been less authoritative about the matter. We have been crusaders, missionaries, determined to save the logical soul of the student from eternal induction. Actually, I believe if the scientific method were presented as the critical content of a positive learning experience; that is, something you can *do*, something you can take apart, ask questions about, criticize, improve on—then one might meet less resistance in getting the student to try it out.

After all, there may be better and as yet undetected methods of dealing with scientific problems—methods of which no one has yet dreamed.

We must maintain an open system; we must "be loose." Therefore, it behooves us to keep our minds open to the possibility of "seeing" things in a different manner. We get new ideas and perceptions from ourselves; and we are unlikely to alter our perceptions until and unless these perceptions frustrate our attempts to do something. (Postman and Weingartner, 1969, p. 90). If we allow students to try and solve a problem by induction and let him fail, then, perhaps he would be willing to relinquish his perceptions and try new ones that might work. (Postman and Weingartner, 1969, p. 90). At any rate; if we expect a good project and project paper, the chances are that we will get it.

In a "mini think-tank" such as we operate at Vernon, Arizona, we emphasize the inquiry method. We help the student decide what is worth knowing and what is relevant to the world of today. We assume that the students will enjoy solving problems and will gain in confidence by learning. We must encourage our students to be aware of the perceptual and cognitive effects of all the new communication media unless we wish them to be at the mercy of those who control the media. And we try to produce people who will be able to deal effectively with the future—a future that is bound to be full of surprises. Certainly, astronomers and physicists are accustomed to seeing things they did not expect to see (Postman and Weingartner, 1969, p. 117): hence, we must train students to operate within an open-system so that they can quickly shift their perceptions and make a choice from several possibilities, not permitting entropy to take them over.

Now, in such an atmosphere, a community of interests arises, a communal feeling of attraction in everyone's problems comes to the fore; and a sense of pride in the accomplishments of every individual and of the school as an entity come into existence. A most remarkable sense of co-operation reigns and one can share ones' troubles and problems with others without fear of mockery.

All of these phenomena are important because they are part of the learning experience, because the students now "see" with a different language, and they act differently. These are subtle, clairvoyant perceptions and penetrations—immeasurable but momentous.

Recently, we have noted an anti-science trend in some of our students. I have the impression that "science" has brought disillusionment or a sense of disappointment and frustration to many, especially to those of the younger generation. Others have discerned this disenchantment (Brooks, 1971). Depending on one's point of view, science is praised or blamed for many of our present blessings or ills.

There is a general and widespread misconception about what science is. Science is neither good nor bad; it is merely a way of looking at things. It is a brand of knowledge that is concerned with establishing and systematizing facts, principles, and methods by means of hypotheses and experiments. It is part of our western culture—just as much a part of it as are rock-and-roll, Mormonism, and apple pie-and-cheese. There can be a *science* of music or history. Science uses the scientific method for understanding and explaining phenomena—especially natural phenomena.

Almost any subject—the explanation of the conduct of aphids, the motions of atomic particles, the migration of birds and animals, how zymogenesis takes place—is a legitimate concern of science. In brief, any observation of the phenomena that surround our daily lives can be grist for the scientific mill—but only so long as the study of those phenomena is concerned with *explanation* that is discovered by the scientific method and/or perhaps by other methods. (For a discourse on the archaeological application of the scientific method, see Watson, 1971)

Sophisticated weapons of war, the pollution of the environment, the space program, agricultural surpluses—all of these things should be viewed as products of technological applications of science—not as “science” itself. These examples demonstrate how science can be used to serve a limited technological “progress,” rather than to contribute to the improvement of life for mankind. Any scientific break-through can conceivably be used against man by means of technology. But long before the scientific revolution started (about 300 years ago) there had been a continuous technological evolution that was spread over hundreds of thousands or millions of years. The discoveries or inventions of fire, the spear-thrower, the bow and arrow, cooking, pottery and basket making, agriculture, irrigation, the wheel—to name only a few—were all made before “science” was even born.

To some extent, we scientists are to blame for the narrow and popular concepts of the nature of science. We have confined ourselves to our personal “ivory towers” and have attempted to maintain an attitude of “pure” science—which is an illusion and no longer tenable. In addition, we have misrepresented science by allowing it to appear to society as an activity that is both unrelated to its cultural milieu and capable of making absolute statements that are *true*. By this I mean that scientists have permitted people to believe what they, the scientists, say is the “truth.” Scientific statements are never true; they are merely provisional statements and generalizations that attempt to make particular data comprehensible and that have to be tested and retested. What is *true* today is false tomorrow. For example, consider the heliocentric conception of the

universe *versus* the geocentric conception *versus* whatever astronomers discover tomorrow.

A scientist—today—may not have a personal, vested interest in the truth or falsity of any hypotheses. We must not allow our activities to be viewed as limited, fixed, irreversible or especially as having a “life” of their own—out of control of either the scientists or of the larger society.

Science is a cultural activity like anything else; it is influenced by, and is influencing the socio-cultural system within which it is practiced. As scientists and teachers, we feel that we must respond to an anti-scientific trend. We must find ways to demonstrate to our students and others that science has positive potentials if it serves humanity, since we can not morally support research without regard to what effect it will have on society.

In our “New Perspectives” school-field-station in Arizona, we try to present science as an “open” system and without “true” or “right” answers. Previously made generalizations are all open to challenge. We encourage students to investigate problems, solutions to which will help make the complexities of the present as well as the prehistoric world more understandable.

Especially in social science, science must be relevant to events of the contemporary world and must bear social responsibility. The old dogma that public issues are beyond our competence is myopic and indicates an out-dated sterile professionalism that is totally irresponsible. A value-free anthropology is a myth. The search for scientific understandings cannot be carried on in isolation from reality.

ACKNOWLEDGEMENTS

I owe great thanks to Postman and Weingartner and to George B. Leonard for inspiration.

III

Preliminary Comments on the Alluvial Chronology of the Hay Hollow Valley, East-Central Arizona

by

DANIEL C. BOWMAN

The Hay Hollow Valley is located near the southern edge of the Colorado Plateau in east-central Arizona (see Frontispiece). The crests of the volcanic White Mountains lie about 30 miles to the south, though there are lava tongues and late Cenozoic cinder cones much closer. The headwater valleys of the Hay Hollow Wash are located about 15 miles northeast of Show Low, Arizona. From there the valley can be followed, essentially northward, to its junction with the Little Colorado River, about 30 miles upstream from Holbrook.

In the Carter Ranch research area the Hay Hollow Valley is primarily cut into sandstones and shales of the early Triassic Moenkopi Formation. On the west side of the valley these sedimentary rocks form a series of structural benches up to mesa tops at over 5,800 ft. elevation. A basalt-capped mesa runs along the east side of the valley, constricting its upper reaches for several miles before the valley opens for the remainder of its course down to the Little Colorado. The steep, unbroken slope of this mesa rises directly from the valley floor, at roughly 5,600 ft., to an elevation of over 6,000 ft. In the bottom of the valley several meters of Late Holocene (Recent) alluvium are exposed along arroyo walls.

A general idea of the climate of the area today can be reasonably based on the weather records from Snowflake. The region is semi-arid with a summer dominant rainfall pattern. The average annual rainfall is just over 12.5 in., with 50 percent of this coming during torrential thunderstorms in July, August, and September; the remainder is evenly distributed over the other nine months. The mean daily maximum and minimum temperatures for January are 47.0 and 16.1° F., for July, 89.9 and 54.7° F. The average number of frost-free days is 132.

The soils throughout the research area show limited horization, whether on top of the basalt mesa, on sandstone and shale slopes, or on the flat alluvial bottomlands. The vegetation mat is quite discontinuous and there are substantial areas of exposed bedrock. The area is overgrazed and during the summer months erosion is very active, both in the form of sheet wash and gullying. Two distinct vegetation communities can be identified: 1.) an upland piñon-juniper woodland zone, occupying the mesa tops and slopes, and 2.) a lowland saltbush-grassland zone with scattered shrubs and short bunch-grass on the alluvial deposits. On the basis of a systematic ecological survey of the area (Zubrow, 1971a, p. 168 ff), a number of present day micro-habitats were defined and ranked in terms of carrying capacity.

A substantial amount of archaeological reconnaissance and excavation has been done in this area (Martin, Rinaldo *et al.*, 1962, 1964; Hill, 1966, 1968, 1970a; Martin, Hill *et al.*, 1967; Leone, 1968; Plog, 1969; Longacre, 1970; Zubrow, 1971a, b, and this volume). However, the only detailed studies of past environments of the valley have been in the field of palynology (Schoenwetter, 1962; Hevly, 1964; Bohrer, 1966, 1968, 1972; Hill and Hevly, 1968; Dickey, 1971). Virtually all of this work has dealt with pollen from samples taken from cultural contexts. On the basis of consistencies in pollen spectra from many archaeological sites and close relationships to pollen and tree-ring chronologies from other areas in the Southwest, Hevly (1964) and Schoenwetter (1962) have felt justified in outlining a general climatic history for the area. At the present time this climatic chronology cannot be related to other environmental variables, and it is this fact which stimulated the research being reported here.

The general objectives of the geomorphological field work are to establish a local alluvial chronology and to obtain a partial understanding of the environmental conditions which prevailed during the time of pre-historic occupation. It is hoped that this study will therefore provide a broader framework within which to analyze the archaeological data now available from the Hay Hollow Valley. Important changes in population, settlement pattern, food resource base, social organization, and other variables have been well documented, but the relationships of these changes to the changing physical environment remain to be investigated.

The present research is only in its beginning stages. An initial season of field work has been completed and laboratory analyses of sediment samples are now in progress in the Geomorphology Laboratory of the Department of Geography, the University of Chicago. During the first season attention was concentrated on the Hay Hollow Valley in the archaeological research area. Reconnaissance was done in the valleys of

larger through-flowing streams (such as Silver Creek and the Zuni and Little Colorado Rivers) and these investigations will be expanded in the future in order to tie the local situation into the larger regional setting of the southern Colorado Plateau.

TABLE 1. A tentative alluvial sequence of Hay Hollow Valley.

10. Modern dissection (to a depth of up to 6 m.).
9. Stabilization, possibly with some minor channel filling.
8. Deposition of Younger Fill(s) (up to 4.5 m. in thickness.).
7. Intensification of downcutting (almost to modern channel level).
6. Dissection of Older Fill.
5. Stabilization, with regrading of Older Fill surface, soil formation and minor channel filling.
4. Minor channel cutting (to a depth of at least 1 m.).
3. Deposition of Older Fill, with two subunits (up to 6 m. in thickness), forming a terrace 6-6.5 m. above modern channel level.
2. Extensive dissection and denudation of Oldest Fill.
1. Deposition of Oldest Fill (over 3 m. in thickness).

A general sequence of events in the local area can be tentatively summarized as follows, employing a provisional, descriptive terminology:

1. Deposition of Oldest Fill. Only traces of the oldest, recognized alluvium are preserved in the headwaters region and against the side of the basalt capped mesa on the east side of the valley. Exposures of up to 3.0 m. in thickness have been found, consisting of semi-consolidated fine sand and silt. Weathering of this alluvial material has produced a carbonate horizon with subangular blocky structure as well as carbonate staining, concretions, and root casts. The tops of these exposures are approximately 8.0 m. above the floodplain of the valley and are often veneered with colluvial debris.

2. Long period of erosion and removal of virtually all of the Oldest Fill.

3. Deposition of Older Fill. The Older Fill is the principal alluvium in the valley, with a thickness of at least 6.0 m., and forming a terrace at about 6-6.5 m. above the modern arroyo bed. It is predominantly a thin to massive-bedded, semi-consolidated reddish-brown sand and silt. Basal gravels and/or conglomerates of up to 1.0 m. or more are widespread. The gravels are composed of local basalt and a variety of lithologies derived from Mesozoic conglomerates and Tertiary river gravels. Two units of Older Fill have been recognized. When they occur together the contact between them is diffuse and irregular. The lower unit, with or without basal gravels, consists of up to as much as 5.0 m. of current-

bedded sands, silts, and clay. The upper unit is composed mainly of massive fine silts and may have a maximum thickness of over 2.0 m.

4. Channel cutting with a vertical amplitude of at least 1 m.

5. Relative stabilization, with weathering and soil formation, regrading of the Older Fill surface, and deposition of fill in channels that cut across this surface.

6. Initiation of a new episode of channel cutting. During this and the following period substantial amounts of the Older Fill were removed over much of the area.

7. Continuation and intensification of downcutting. Incision into the basal gravels of the Older Fill, almost to the depth of the modern arroyos.

8. Deposition of the Younger Fills, including channel-fill units, well-stratified alluvial deposits of silts and sands, and loose, poorly-stratified colluvial veneers, all disconformable over the Older Fill. The Younger Fill frequently indicates the presence of two units with a diffuse contact between them. The lower one generally consists of 50-70 cm. of well-stratified, partly laminated, sand and silt, occasionally with a basal pebble bed. It grades into the upper unit which may be expressed in a number of different lateral facies. The range of expression is from 10-20 cm. of loose, colluvial wash to 1 m. or more of moderately to poorly stratified sands and silts.

9. Short period of relative stabilization, with modest soil formation and possibly minor deposition of channel fills.

10. Modern arroyo cutting, to a depth of 5-6 m. Dune formation and fan alluviation are also apparent locally. The arroyo has cut down to bedrock in many places and while not apparently incising bedrock at present, it is actively under-cutting its banks. Headward erosion by small gullies and tributary arroyos is proceeding rapidly.

The interval between the deposition of the Older and Younger Fills has been divided into four periods (Nos. 4-7). The first two represent a time of minor channel-cutting (phase 4), followed by relative stabilization marked by the regrading of the Older Fill surface and limited channel filling (phase 5). On the basis of superficial observations, there are suggestions of intensive settlement on the low terraces above these channels. A new episode of arroyo cutting (phase 6) followed, presumably in relation to changes in local edaphic and climatic conditions. There is the provocative possibility that the construction of irrigation canals to bring water from the stream valley on to the valley bottom lands was a response by the prehistoric inhabitants to this situation. Then, as downcutting

continued (phase 7), the irrigation canals were eventually abandoned. This is a very tentative reconstruction; the relationship of the canals to the alluvial history will be an important focal point in future investigations.

The Younger Fill is in most cases a relatively minor unit, but it appears to be one with multiple facies; or even several distinct stratigraphic subdivisions. In all cases there is a disconformity with a sharp contact between the Older Fill and the younger deposits. The maximum expression of younger alluvia, to a total thickness of up to 4.5 m., occurs in exposures that include channel fills. The channel fills proper, as much as 2.0 m. thick, consist mainly of sands with some gravels and finer materials, and are frequently cross-bedded. These fills may be overlain by two horizons of more extensive alluvial deposits. The lower of these is usually composed of loose, well-stratified, horizontally bedded, reddish-brown sands and silts, less than 1 m. in thickness. Above a diffuse contact, the upper horizon, on occasion as much as 2.0 m. or more in thickness, is characterized by weakly stratified sands. This last subunit may change facies laterally to a brownish colluvial wash of several tens of centimeters in its maximum expression. After deposition of at least the major portion of the Younger Fills there appears to have been another period of relative stabilization prior to the initiation of the modern episode of arroyo cutting.

Traces of an apparent Oldest Fill unit have only been preserved on the far sides of the valley. Detailed laboratory analysis and additional field work will be necessary before these deposits can be properly evaluated. Other points for intensive future study in the archaeological research area will include the interdigitation of alluvium with lateral alluvial fans, especially those below the basalt-capped mesa, detailed examination of exposures of beds that appear to be intensively weathered, and continued investigation of the stratigraphic position of multiple occurrences of juniper trees buried by rapid alluviation. It is hoped that this study will open new avenues for archaeological research in the Hay Hollow Valley and make it possible to relate additional aspects of the prehistory of that area to a larger, regional context.

ACKNOWLEDGEMENTS

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IV

Ecological Perspectives in the Hay Hollow Valley

by

EZRA B. W. ZUBROW

INTRODUCTION

Recently, archaeological and anthropological studies have used environmental or ecological variables as independent variables in explanations of cultural process (White, 1959; Müller Beck, 1967). These variables have been considered individually or interconnected in complex ecological systems (Zubrow, 1971b). The data used to measure the variables have shown considerable variation in content and in descriptive adequacy. Fine-scale variable measurement (Conklin, 1957) exists side by side with studies of less environmental detail.

Although the Hay Hollow Valley has been the laboratory for archaeological fieldwork of the Southwest Archaeological Expedition for 15 years, only relatively recently has a concerted effort been made to examine rigorously the prehistoric environment. This interest in the quantitative examination of the prehistoric environment reflects the shift in archaeological theory from a study in time-space systematics to a study of cultural processes. Once cultural processes, rather than events, become the primary focus of research, then the correlation and structural similarity of cultural and ecological processes becomes important.

In order to systematically begin an analysis of ecological processes a series of palynological studies were undertaken by Vorsila Bohrer (1968) and Richard Hevly (1964). These turned out to be very successful direct monitorings of the prehistoric environment. Environmental changes and cultural processes were correlated at the site level by Hill and Hevly (1968) and at the regional level by Zubrow (1971b). These palynological studies resulted in a series of attempts to develop (Driskell, 1969; Gregory, 1969) and to test (Plog, 1969) explanations in which the environment was an independent variable. Also, the same data were very useful in interpreting cultural variables such as economic variation and demographic distribution.

However, new problems, such as the simulation of environmental, ecological, and cultural systems, has necessitated the gathering of new data. These data, although both quantitatively and qualitatively different from the palynological studies, continue in and reinforce the systematic analyses of ecological processes begun by the palynologists. Thus, this brief study attempts to serve several functions at once. One function is to partially describe the ecological environment of the Hay Hollow Valley. Another of the study's functions is to resolve certain problems dealing with the analysis of the Valley's microhabitats. Finally, it represents a report on several methodological innovations.

PROBLEMS

During the summer of 1970 Richard Hevly and I directed an ecological survey in the Hay Hollow Valley. Its purpose was threefold. First, we wanted to determine if the topographic and soil zones correspond to differences in flora and fauna. If so, were these differences sufficiently great as to be labeled different microhabitats? Second, we wished to determine the actual amount of resources and resource productivity available to the prehistoric population. Third, we wanted to obtain these resource figures with sufficient representative accuracy as to be usable in the simulation of a model of carrying capacity as a dynamic equilibrium system (Zubrow, 1971b). The ecological survey consisted of eight stages.

Stage 1 was the accurate determination of the topographic and soil zones which was accomplished by using aerial photographs, geological and soil maps, and field survey techniques.

Stage 2 was the plotting and field location of a representative sample of nested quadrats in each potential microhabitat for floral analysis.

Stage 3 was the plotting and field location of a series of representative transects for each potential microhabitat for faunal analysis.

Stage 4 was the initial gathering of floral data. This consisted of measuring by genera the number and size of trees in the 30 m. quadrats; the number and size of bushes and shrubs in the 10 m. quadrats; and the number and size of herbs and grasses in the 1 m. quadrat.

Stage 5 was the gathering of animal data along the transects by five members of the expedition moving simultaneously along the transect identifying all genera of mammal, bird, and reptile life by number for 25 days, as well as by live trapping.

Stage 6 was the final gathering of floral data. This consisted of returning to each of the 1 m. quadrats (which had been initially clipped at

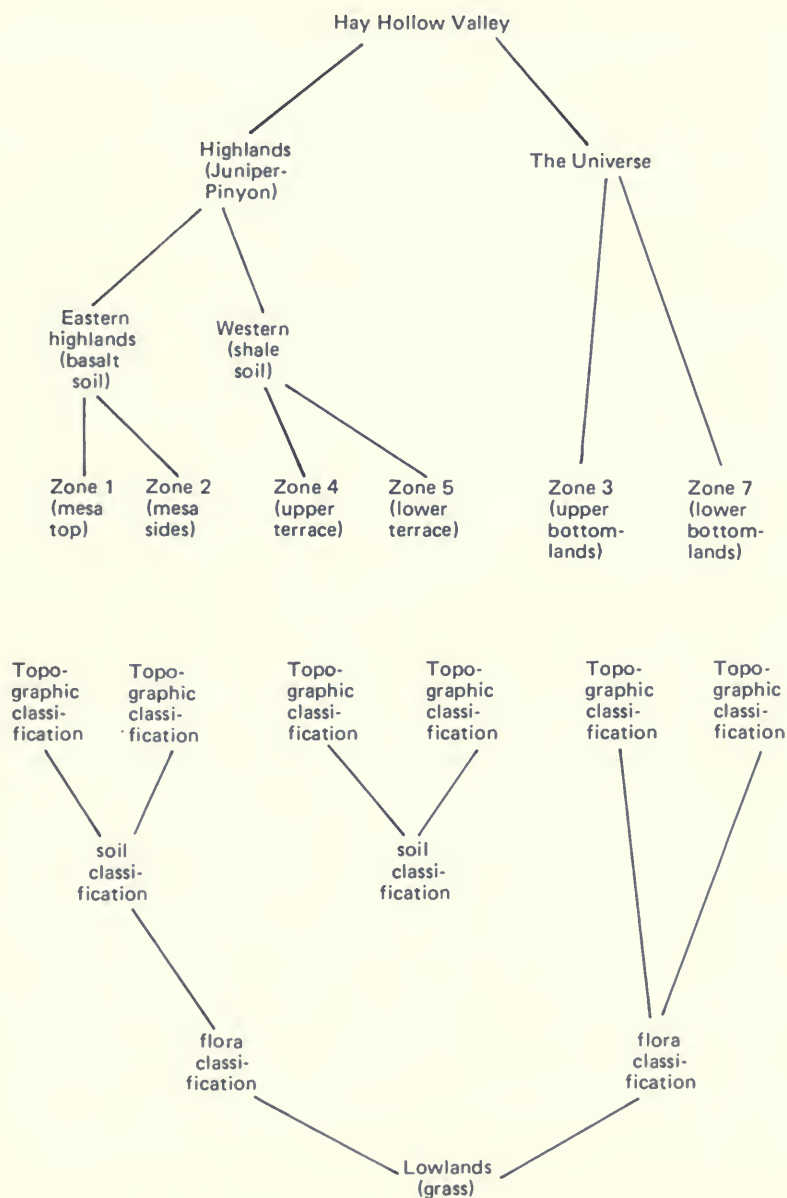


FIG. 1. Preliminary classification of microhabitats in the Hay Hollow Valley.

stage 4) and reclipping all growth. Both sets of clippings were sent to Northern Arizona University where their weights were measured by genera.

Stage 7 was the planting and harvesting of three plots of corn for agricultural data—one tended and irrigated, one near water, and one plot in an arid region.

Stage 8 was the relating of modern resource data to past resource data.

Although at first glance this might seem to be a reasonably complete resource analysis, it is incomplete. A complete analysis would have to control both temporal and spatial data for all of the variables which impinge upon the modern and prehistoric use of the resources in the microhabitats (Hevly, 1970). The incompleteness of the available data should be kept in mind when judging the finished study.

The eight stage research design was an attempt to maximize information with minimum financial expenditure and maximum utilization of available talent.

Stages 1, 2, and 3

Attempts were first made to determine microhabitats in the Hay Hollow Valley by Schacht in 1968. On the basis of U.S.G.S. aerial photographs he differentiated two major ecological zones—a highland zone with juniper pinyon and a lowland zone which was predominantly saltbush grasslands (fig. 1). He then divided the highlands into two geographic and edaphic zones. The eastern highlands had soils derived from basalt, while the western highlands had soils derived from shale. Topographically he then subdivided the eastern highlands into the mesa top (microhabitat I) and the mesa sides (microhabitat II) while the western highlands were subdivided into an upper (microhabitat IV) and lower (microhabitat V) terrace which Bowman has called benches. The lowlands, although not divided on geographic or edaphic criteria were divided topographically into upper (microhabitat III) and lower (microhabitat VII) bottomlands. He felt, but was unable to show, that microhabitats I and II, on one hand, and microhabitats IV and V, on the other, might only differ in minor detail.

The Strategic Air Command was kind enough to present the Southwestern Archaeological Expedition with a new set of aerial photographs which had a much finer degree of detail and resolution than the U.S.G.S. photographs. The United States Soil Conservation Service provided us

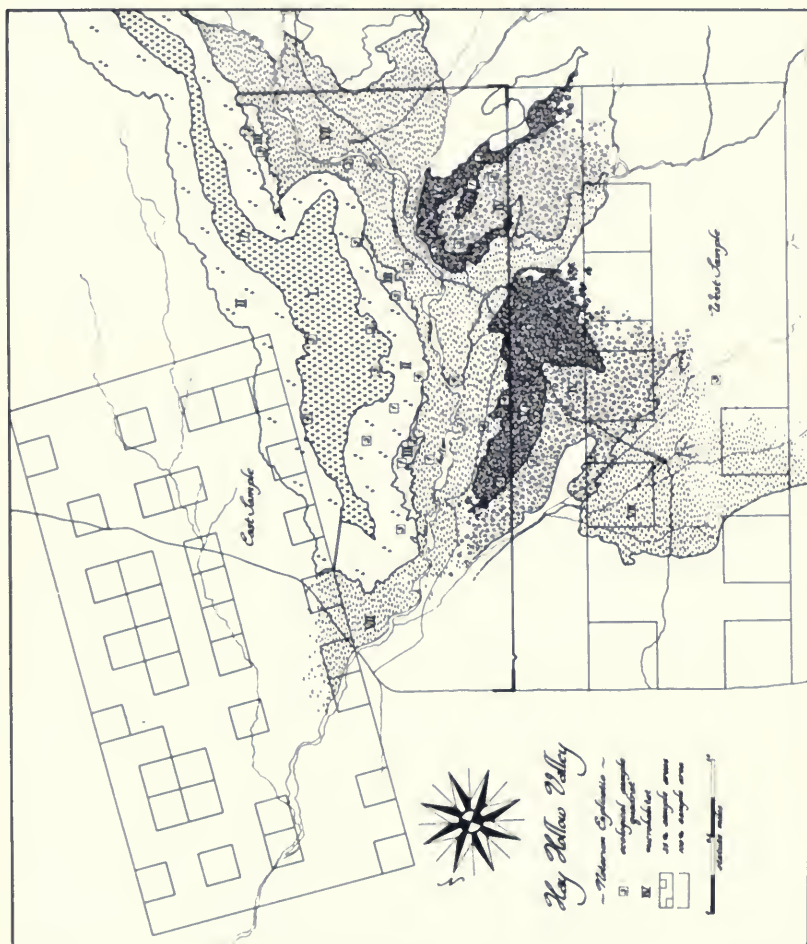


FIG. 2. Map of Hay Hollow Valley showing microhabitats and ecological survey sample units.

with detailed soil maps of the area. With this information we went back to the field and made corrections in Schacht's original formulation. Most of these corrections were minor and previously dealt with the microhabitats IV and V boundaries and the microhabitat III boundaries. The corrected version is shown in the map (fig. 2).

It was decided to take a series of floral samples from each of the potential microhabitats which would serve two functions. First, it would provide Hevly and myself with quantified data which would allow the statistical differentiation of the actual present day microhabitats. Second, it would allow us to quantify the actual present day resource potential of the microhabitats.

One might object to this procedure as having no relevance to the past. Two answers are possible to this objection. First, although one probably would not want to suggest that the floral samples from today are exactly equivalent to those of the past, it does give a reasonable estimate. In any case, it is far more accurate than the usual generalized archaeological statements about the environment.

Second, it is possible to relate the modern environment and floral samples to the past by using environmental indices such as palynology (see the discussion of stage 8).

A series of problems had to be solved before it was possible to know that our floral samples were representative. Line transects are the easiest and quickest sampling procedure for estimates of density, frequency, and cover. However, there is some question whether it would give an accurate estimate to the plant material due to the random aggregation of plants. Since trees show the most variation in aggregation in small areas, it was decided to test the accuracy of transect data by comparing it to quadrat data on trees. Using the SAC high resolution photograph of the Broken K area, we randomly selected 10 samples. Each sample consisted of four nested quadrats. The quadrats were from smallest to largest—17 m. \times 17 m., 34 m. \times 34 m., 51 m. \times 51 m., and 68 m. \times 68 m. The line transects were two boundaries of the quadrat at right angles to each other for all four nested quadrats, (fig. 3). Thus, the transect length for the smallest quadrat was 17 m., for the next larger quadrat 34 m., etc.

The 17 m. transects compared to the 17 m. quadrats showed an average error¹ of -62 percent calculated by summing the individual

¹ The percentage error was calculated as:

$$\% \text{ error} = \frac{(\text{transect estimate} - \text{actual quadrant number of trees})}{\text{actual quadrant number of trees}/100}$$

errors and averaging. Similarly the 34 m. transects, the 51 m. transects, and the 68 m. transects showed in comparison to their respective quadrats errors of -34 percent, -33 percent, and -22 percent. In all cases the 90° transect method seriously underestimated the number of trees and was thus rejected.

A second sample was taken in order to test if line transects selected on a criterion of at least two trees in the first 30 m. gives a more accurate and representative estimate (see fig. 3). This method is also rejected for the error is even greater than the first method with average errors of -79 percent, -41 percent, -44 percent, and -46 percent for the 17m., 34 m., 51 m., and 68 m. transects, respectively.

Having rejected both line transect methods, it was decided to attempt to use quadrats as a method of sampling. The question which arose was, "what is the smallest quadrat which would give valid representative data but which was small enough to be handled by the expedition's resources?" In order to determine the representative qualities of the sample, it was assumed that if the quadrat gave an accurate estimate of the number of trees in an area of more than an acre, it was representative and sufficiently accurate. These estimates and errors¹ were calculated for both sets of sample quads which were originally used for testing the two types of transects. The resulting errors for 17 m. × 17 m., 34 m. × 34 m., and 51 m. × 51 m. quadrats are 101 percent, 20 percent, and -4 percent for the first sample of ten quadrats and 72 percent, 31 percent, and 10 percent for the second sample of ten quadrats.

¹ The largest quad, 68 m. × 68 m., is more than an acre. The smallest quad is one-sixteenth of the largest, the next largest is one-fourth, and the next is nine-sixteenths. The estimate is then defined as $E = 16x$, $E = 4x$, $E = 16/9x$ for the 17 m. × 17 m. quad, for the 34 m. × 34 m. quad, and the 51 m. × 51 m. quad where:

E = is the estimate to be compared with the 68 m. × 68 m. quad

\times = is the number of trees found in the smaller quads from which the estimate is being made.

$$\text{The error is calculated as } ER = \frac{\sum_{n=1}^{10} (E - K)}{\sum_{n=1}^{10} K} \times 100 \text{ where:}$$

Er = error

E = estimate from a particular quad size excluding 68 m. × 68 m. quad

K = is the actual number of trees in the 68 m. × 68 m. quad

n = the number of samples

It would appear that the quadrats are capable of producing better estimates. However, it should be noted that in order to do accurate estimating it takes far larger quadrat size than the professional biologists and ecologists usually deem necessary. For example, Smith (1966) claims:

The size of the quadrat must be adapted to the characteristics of the community. The richer the flora, the larger or more numerous the quadrats must be. In forests, quadrats of one-fifth acre are established to include the trees, while smaller quadrats can be used to study shrubs and understory. For the latter as well as grass cover, quadrats of one square meter are the usual size.

In other words, for rich flora such as forests one shouldn't have to use quadrats of more than one-fifth acre. Hevly (1970) notes that common nested quadrat sizes are 10 m. \times 10 m. for trees, 4 m. \times 4 m. for shrubs, bushes, and understory, and 1 m. for grasses. Since these values are based on larger studies where quadrat size was correlated with many more than 60 quadrat estimates, Hevly and I decided that it would be more appropriate to compromise our large quadrat size with the professional values. Thus, we used quadrats of 30 m. \times 30 m. for trees, 12 m. \times 12 m. for shrubs, bushes, and understory, and 1 m. \times 1 m. for grasses. Five of these were randomly located in each potential microhabitat. Their exact location is plotted on the map (fig. 2). However, one caveat should be noted. Namely, this size quadrat may have considerable error which may partially vitiate the use of the data (up to about 25 percent).

In order to determine the amount of faunal life a series of road transects were devised which cut across the potential microhabitats in addition to live trapping. These are just two of many potential methods which could have been used. Smith (1966) suggests sample plots, strip census, mark-recapture method, the population removal method, live trapping, and pellet counts as alternative methods. Each of these have assets and disadvantages. We chose the road transect method primarily on the basis of logistic ease and time requirements rather than on statistical or sampling reasons.

Since animal data are quite variable, the longer the transect, the more accurate is the relative representation of the population. A trade-off decision was made between the isolation of the transect and the length of the transect. The greater the isolation of the transect the less disturbance of the fauna, but the greater the logistic problems and the less the total length of the transect. Thus to maximize the transect's length they were chosen near back country and ranch roads. It may be reasonably argued that the automobile traffic along these roads would result in a skewed sample. This is probably true. However, two minimizing factors should

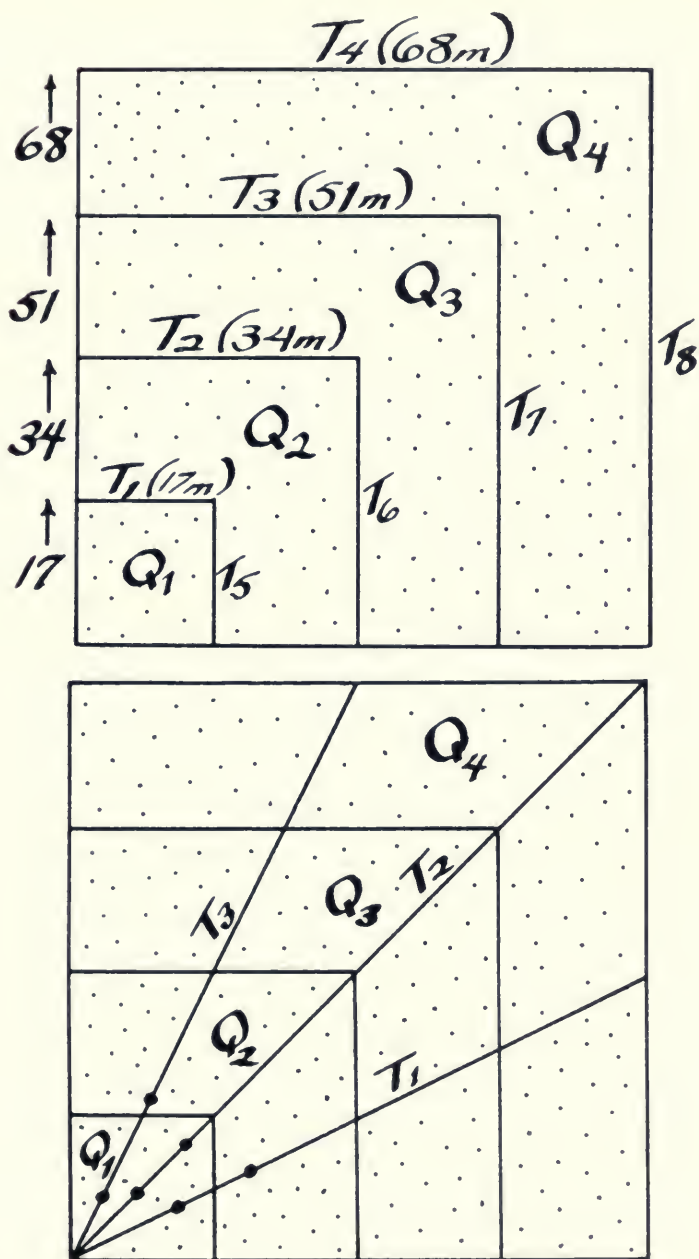


FIG. 3. Transect and nested quadrat configurations.

be noted. One, during prehistoric times when there was a sizeable population in the valley and the animal distribution would have been affected by human activity, the skewed samples may be to a certain degree equivalent. Two, the skewing is considerably less than what might be at first expected since the roads along which the transects were taken have very little traffic. Traffic along the chosen road transects varied from one automobile per four hours to one automobile per two months. The shorter periods were ascertained by observation; the longer periods were determined by the ranch owner.

Stage 4

As previously noted, stage 4 was the initial gathering of the floral data. The number and size of trees were derived from the 30 m. quadrats, the number and size of bushes and shrubs derived from the 10 m. quadrats and the number and size of the herbs and grasses from the 1 m. quadrat. Publishing difficulties prevent me from presenting the tables of plants by sample quadrat for each microhabitat. They are, however, available upon request. Table 5 shows the summation of the number of plants from the five quadrats per potential microhabitat.

In order to determine the reality of the zones two tests were made. First, a chi square test was done using the 37 species of plants. The sum number of each species for the five quadrats in each microhabitat was used. The observations thus formed a matrix of 37 species by six microhabitats. The resulting chi square was significant at greater than the .01 level. However, one is overestimating the χ^2 value because of the large number of cells and frequencies which are less than 5. Aggregating the data into trees, shrubs, and herbs and grasses provides a matrix with no frequency below 30. This χ^2 is also significant well above the .01 level. Thus, one may conclude that the six microhabitat distributions taken as a group show the result of factors other than chance variation as well as being as a group independent of each other *vis à vis* the distribution of plants.

The second test was done in order to tell if there were significant relationships between the individual microhabitats when analyzed one against the other, rather than as a group. In order to do this, a series of Pearson Product Moment Correlation coefficients¹ was run between the various microhabitats. The correlation coefficients using the sum data is

¹ The Pearson Product Moment correlation coefficient was calculated according to the following formula.

in the following table. By sum data I mean that the *i*th case of the two variables, x_i and y_i , are the total number of plants of one species in the five groups of three nested quadrats in microhabitat *x* and in the five groups of three nested quadrats of microhabitat *y*.

TABLE 2. Correlation coefficients, *r* and *rho*, of total numbers of plants by species by microhabitat.

Microhabitats	I	II	III	IV	V	VII
	<i>r</i> (<i>rho</i>)	<i>r</i> (<i>rho</i>)	<i>r</i> (<i>rho</i>)	<i>r</i> (<i>rho</i>)	<i>r</i> (<i>rho</i>)	<i>r</i> (<i>rho</i>)
I/	xxx	xxx	xxx	xxx	xxx	xxx
II/	<u>.42</u> (.32)	xxx	xxx	xxx	xxx	xxx
III/	<u>.49</u> (.58)	<u>.69</u> (.40)	xxx	xxx	xxx	xxx
IV/	.16 (.41)	<u>.85</u> (.65)	<u>.54</u> (.47)	xxx	xxx	xxx
V/	.17 (.25)	<u>.80</u> (.48)	<u>.41</u> (.34)	<u>.78</u> (.56)	xxx	xxx
VII/	<u>.66</u> (.35)	.31 (.39)	<u>.55</u> (.37)	.09 (.28)	.11 (.27)	xxx

The underlined values are the most significant. If one uses mean data rather than sum data the correlation coefficients are as follows. By mean

(continued from p. 26)

$$r = \frac{n \sum_{i=1}^n x_i y_i - \left(\sum_{i=1}^n x_i \right) \left(\sum_{i=1}^n y_i \right)}{\sqrt{n \sum_{i=1}^n x_i^2 - \left(\sum_{i=1}^n x_i \right)^2} \cdot \sqrt{n \sum_{i=1}^n y_i^2 - \left(\sum_{i=1}^n y_i \right)^2}}$$

where: *r* = is the correlation coefficient

n = is the number of cases

i = is the counter of the number of cases

x_i = is the *i*th value of the variable *x*, one of two variables being correlated

y_i = is the *i*th value of the variable *y*, one of two variables being correlated

In order to test the significance of *r*, one assumes the null hypothesis that the deviation from the expected value of *r* is sufficiently small that it could have happened purely by chance. In other words, we are assuming what we wish to disprove. The proof of the hypothesis of meaningful correlation is the disproof of the null hypothesis. Under the null hypothesis (*P* = 0) one may state that the sampling distribution of *r* is approximated by a normal curve. Its mean and standard deviation are then equal to *m* = 0 and $\sigma_r = \frac{1}{\sqrt{n-1}}$

Using .05 for the level of significance, it is possible to state that the null hypothesis is disproved, or the coefficient of correlation is significant, if *r* exceeds 1.96 σ_r or is smaller than -1.96 σ_r . One must reserve judgment if *r* falls between these two values. In our case where the number of species is 38, σ_r is equal to .164. 1.95 (.164) is equal to .3222. Thus, if *r* is greater than .3222 or less than -.3222, the null hypothesis is disproved and *r* is a significant correlation.

data I mean that x_i and y_i are the mean number of plants of one species for the five groups of three nested quadrats in microhabitat x and in the five groups of three nested quadrats of microhabitat y.

TABLE 3. Correlation coefficients, r, of numbers of plants by species by microhabitat using mean data.

Microhabitats	I	II	III	IV	V	VII
I/	xxx	xxx	xxx	xxx	xxx	xxx
II/	<u>.43</u>	xxx	xxx	xxx	xxx	xxx
III/	<u>.49</u>	<u>.61</u>	xxx	xxx	xxx	xxx
IV/	.16	<u>.81</u>	<u>.54</u>	xxx	xxx	xxx
V/	.16	<u>.80</u>	<u>.41</u>	<u>.78</u>	xxx	xxx
VII/	<u>.68</u>	.31	<u>.55</u>	.08	.08	xxx

It is interesting to note that there is a high degree of similarity between the two sets of correlation coefficients even to the extent that they have identical combinations which are significant. From these correlation coefficients it is possible to conclude the microhabitats II, IV, and V have a high degree of similarity. r^2 is considered to be a measure of the amount of variability explained. r^2 for II-IV, II-V, and IV-V is .66, .64, and .61 respectively. This means that 66 percent of the variability in microhabitat IV is explained by microhabitat II. The other values explain the variability for II-V, and IV-V similarly. These microhabitats show the highest degree of similarity of all the microhabitat combinations. It seems reasonable to conclude on the basis of correlations whose values are approximately .80 that the three microhabitats show sufficient similarity as to be called one microhabitat. This is, of course, solely on the basis of flora.

If one is unwilling to accept the parametric and distributional assumptions of Pearson's Product Moment for the plant communities, one may correlate the samples from the nested quadrats with Spearman's rank order correlation coefficient.¹ There are differences as would be expected,

$$^1 \text{ Spearman's rank order correlation coefficient is usually calculated as } r_s = 1 - \frac{6 \cdot \sum D^2}{n(n^2 - 1)}$$

where:

ρ = Spearman's rank order correlation coefficient

n = the number of cases which are ranked

D = the difference between the ranks for each paired case

Appropriate corrections in both the formula and the rankings were made for the data which resulted in tied ranks. In order to determine significance the t test was used, $t = \frac{\rho}{\sqrt{1-\rho^2}} \sqrt{n-2}$.

since $n > 25$, ρ is significant at the .05 level when $t > 2.03450$ and at the .01 level when $t > 2.73329$ for the plant data.

as well as essential similarities. The correlation coefficients of microhabitat II-IV, II-V, IV-V are among the four highest. The important difference is that there is also a high coefficient for I-III. Thus, using r one could suggest a combined II, IV, V microhabitat and I, III, and VII as separated. Using ρ one could suggest two combined microhabitats, II-IV-V and I-III with VII separated. However, it should be noted that the ρ coefficients are low.

Stage 5

Stage 5 was the gathering of animal data from the transects and from live trapping. Each transect covered a width of one-tenth mile. The total transect area represented is:

microhabitat I	1.380 sq. miles
microhabitat II144 sq. miles
microhabitat III110 sq. miles
microhabitat IV	1.650 sq. miles
microhabitat V544 sq. miles
microhabitat VII	3.054 sq. miles
microhabitat VIII	6.9 sq. miles

Microhabitat VIII is a continuation of potential microhabitat IV towards Snowflake.

Tables 6 and 7 are the summation of the transect data and include also the density data which was calculated by the summation data divided by the microhabitat areas. Pearson Product Moment correlation coefficients were calculated on the density data by microhabitats. The results below show that there is only significant² correlation of sufficient size to suggest that two microhabitats are the same animal microhabitats. These are microhabitats IV and VIII.

TABLE 4. Correlation coefficients, r and ρ , of animal densities by microhabitat.

Microhabitats	I	II	III	IV	V	VII	VIII
	r (ρ)	r (ρ)	r (ρ)	r (ρ)	r (ρ)	r (ρ)	r (ρ)
I	xxx	xxx	xxx	xxx	xxx	xxx	xxx
II	<u>.42(.45)</u>	xxx	xxx	xxx	xxx	xxx	xxx
III	<u>.01(.10)</u>	<u>-.01(.19)</u>	xxx	xxx	xxx	xxx	xxx
IV	<u>.38(.45)</u>	<u>.31(.40)</u>	<u>-.09(-.08)</u>	xxx	xxx	xxx	xxx
V	<u>.58(.33)</u>	<u>.35(.14)</u>	<u>.02(-.01)</u>	<u>.43(.39)</u>	xxx	xxx	xxx
VII	<u>.47(.32)</u>	<u>.15(.22)</u>	<u>.17(.18)</u>	<u>.35(.45)</u>	<u>.63(.42)</u>	xxx	xxx
VIII	<u>.38(.29)</u>	<u>.32(.36)</u>	<u>-.03(-.10)</u>	<u>.85(.40)</u>	<u>.47(.13)</u>	<u>.42(.20)</u>	xxx

²or was .145 and r had to be greater than .284 or less than $-.284$ to be significant at the .05 level.

Since microhabitat VIII is a continuation of microhabitat IV, it is not a major surprise that the two microhabitats correlate sufficiently as to be described as essentially the same. Using the r^2 value both II and IV explain approximately 72 percent of the variability of each other. Not making the parametric assumptions of r and using ρ again, one notes that the coefficients continue to be low.¹

What is interesting is that unlike the floral microhabitats—microhabitats I-VII do not correlate with each other sufficiently that one could claim without stretching the information from the coefficients that any combination of them are one microhabitat. Thus, one is left in the position of having four probable floral microhabitats and six faunal microhabitats. This difference should have settlement dispersion consequences. Namely, that during periods of hunting and gathering economies when a greater dependence upon hunting was necessary, there should be a greater dispersion of sites across the microhabitats.

Hevly directed a hunting and trapping expedition which took place between September 3-6 and 19-20. Both vertebrates and invertebrates were collected from three areas. Trapping habitat 1 was microhabitat IV and V; trapping habitat 2 was microhabitat VII and microhabitat III; and trapping habitat 3 was microhabitat I and II. Two sets of results from the vertebrate survey are relevant. First, the similarity coefficient shows that the three trapping habitats are distinct which agrees with the transect results. Second, since the animals were trapped and weighed, it is possible to find the amount of vertebrate biomass that each of Hevly's habitats is capable of supporting. Habitat 1, the pinyon juniper woodland, had six species trapped which were distributed .091 g/m² of herbivores and .003 g/m² of carnivores. Habitat 2, the grasslands, contained 11 species which were distributed .327 g/m² to herbivores and .033 g/m² to carnivores. The third habitat, the juniper savanna, contained six trapped species which were distributed .102 g/m² to herbivores and .004 g/m² to carnivores. The invertebrate survey showed for habitat 1, 17 species, .025 g/m² of herbivores, .01 g/m² of carnivores; for habitat 2, 23 species, 1.05 g/m² of herbivores, and .05 g/m² of carnivores; and for habitat 3, 16 species of which .95 g/m² were herbivores and .05 g/m² were carnivores. Finally, it should be noted that for invertebrates that the grassland and pinyon juniper woodland are easily distinguished from each other but both share a compliment of species with the juniper savanna.

Thus to summarize the ecological data up to this point, there are four floral microhabitats and six faunal microhabitats on the basis of quadrat

¹ ρ is significant at the .05 level when $t > 2.01173$ and at the .01 level when $t > 2.68456$ for animal densities.

and transect data (fig. 2). The hunting and trapping data show clear demarcation of at least the three tested habitats for vertebrates but similarity of one habitat to two distinct habitats for invertebrates.

Stage 6

In order to determine the potential resources, the size of the standing crop was determined. Each of the 1 m. quadrats whose species tabulations make up part of Table 5 were clipped during stage 4, and a sample of the species making up the 10 m. quadrats were also clipped. These clippings were sent to Northern Arizona University where their weights by genera per quadrat per microhabitat were measured under the direction of Hevly. The results at the general specific level are on permanent file with Hevly and will not be reproduced here. The summation of the floral results are the following:

Microhabitat I	70.7500 \pm 10.2971 g/m ²
Microhabitat II	36.9900 \pm 2.6608 g/m ²
Microhabitat III.....	26.6060 \pm 11.8317 g/m ²
Microhabitat IV.....	24.2020 \pm 9.9155 g/m ²
Microhabitat V	42.4650 \pm 12.6035 g/m ²
Microhabitat VII.....	62.346 \pm 7.8297 g/m ²

Adding the results of the vertebrate and invertebrate survey to the floral standing crop, it is possible to determine the total standing crop for each zone. These are in order from habitat I to habitat VII—71.856 g/m², 38.096 g/m², 27.066 g/m², 24.556 g/m², 42.819 g/m², 62.806 g/m². In no microhabitat is the faunal biomass more than 2.9 percent of the total biomass and it averages for all microhabitats as 1.5 percent of the total biomass. This indicates that a heavily fauna dependent economy would be severely limited. In fact, it is even more limiting if one calculates the relative food values of the faunal biomass to the floral biomass. Using the 4 kcal/g for floral biomass from Odum (1963) and the 2.16 kcal/g for faunal biomass derived from food composition tables, one finds that the relative food value of the fauna is only 8 percent of the flora.

From the above, one would rank the ecological microhabitats from highest to lowest carrying capacity I, VII, V, II, III, IV. However, this is somewhat misleading. The size of the standing crop influences the capacity to produce but it is not the capacity to produce. It is analogous to capital in a bank. The capital influences how much is produced but the actual production is the result of capital and the interest. The productivity is the amount of renewable growth similar to the interest payments. As long as the capital or carrying capacity stays the same one may drain off interest payments or productivity amounts without affecting the capital or carrying

TABLE 5. Total plant distribution for all quadrats by microhabitat.

Species	Number of plants in all quadrats in microhabitat					
	I	II	III	IV	V	VII
<i>Trees</i>						
Pinyon pine		9		1	3	
Juniper	29	131	26	70	55	1
<i>Shrubs</i>						
Saltbrush (<i>Atriplex</i>)	2	2	8	1	9	30
Sagebrush (<i>Artemisia</i>)	15	40	8	61	22	
Prickly pear (<i>Opuntia</i>)	8	19	13	7	6	5
Cholla (<i>Opuntia</i>)	13	1	3	2	1	4
Barberry (<i>Berberis</i>)			2		3	
Winter fat (<i>Eurotia</i>)	4		1			24
Yucca (<i>Yucca</i>)	1		9	1	2	
Beargrass (<i>Nolina</i>)						3
Other: <i>Lycium</i>	4		3			1
<i>Berberis</i>		1				
<i>Ephedra</i>		1		7	1	
<i>Echinocereus</i>		1	1			
<i>Amorpha</i>			1			
<i>Forestiera</i>			1			
Cliff rose				9	13	
Ironwood					3	
<i>Herbs</i>						
Grass: <i>Aristida</i>	12		15	1		
<i>Agropyron</i>		6				
<i>Bouteloua</i>	334	61	34	14	12	92
<i>Hilaria</i>					33	
<i>Muhlenbergia</i>	19	1	35	19	3	1
<i>Sporobolus</i>		35	33	20	10	65
Other: <i>Aster</i>	35				1	
<i>Boerhaavia</i>				2		
Goosefoot (<i>Chenopodium</i>)						
red mist	1					
Snakeweed (<i>Gutierrezia</i>)		15		1	1	3
Buckwheat (<i>Eriogonum</i>)				2		
Locoweed (<i>Astragalus</i>)			1		2	
Other: Plantain	1					
Wormwood (<i>Artemisia</i>)			4			
<i>Sphaeralcea</i>			1		1	
<i>Cryptantha</i>					5	14
<i>Aster</i>						8

capacity. Similar strictures may be suggested for subsistence economies based on carrying capacity as may be suggested to the banker who is looking for long-term gains with minimum risk, i.e., don't dip into your capital.

In order to determine the productivity of the potential microhabitats, the same areas that were originally clipped, the 1 m. quadrats were reclipped one month later. These second sets of clippings were also sent to Northern Arizona University where they were weighed by genera per quadrat per microhabitat. The genera specific results of this second set of clippings is also being kept by Dr. Hevly on permanent file. The results were:

microhabitat I	10.0800 \pm 2.1513 g/m ²
microhabitat II	12.4000 \pm 2.7746 g/m ²
microhabitat III	14.6600 \pm 2.3986 g/m ²
microhabitat IV	7.1800 \pm 4.2247 g/m ²
microhabitat V	2.1400 \pm 0.8640 g/m ²
microhabitat VII	22.0700 \pm 13.7340 g/m ²

It is important to note that the productivity figures do not exactly correspond to the carrying capacity figures. In order words, because microhabitat I has the largest standing crop does not mean it has the largest productivity. Microhabitat VII has the largest productivity. Unfortunately, the study was not in the field long enough to gather data on animal productivity.

Stage 7

In order to estimate the amount of production possible from agriculture, three plots of corn were planted. Two were planted in microhabitat VII and one in microhabitat IV. One of the two plots in microhabitat VII was located on "dry" land near the Gurley site. By dry I mean its only source of water was rainfall and runoff. The other was located by a water-filled irrigation ditch. In each of these sample plots the same method of planting was used. The grass was cleared for an area of three feet in circumference around each hole in which the corn was to be planted. Then a 1 ft. diameter hole was dug about 6 in. in depth. Into each hole was put 15-20 kernals of variegated, red, green, and yellow Hopi corn. In each of the microhabitat VII plots, five holes were dug and around one hole a protective screening was placed. Each hole was watered and then covered.

The third plot of corn was planted in Mrs. Carter's garden which is in microhabitat IV. Here two half rows of corn were planted by pushing the

corn kernels into the turned soil. Mrs. Carter watered as well as weeded this plot.

The results of this experiment were at best ambiguous. In the plot located in microhabitat VII near the irrigation ditch no corn whatsoever grew. In the "dry" habitat VII plot only one corn plant grew.

The plot which was grown in Mrs. Carter's garden resulted in a full crop producing approximately the same amount of corn as the modern species which were being grown both in the garden and surrounding area. Calculating then solely on the basis of modern conditions for the county and using food composition it is possible to produce 25.43 g/m² of corn kernels which is equivalent to 91.29 kcal/m². This must be considered as productivity since corn is an annual plant. Comparing the corn productivity value with the total natural floral productivity value of the most productive microhabitat, microhabitat VII, it is important to note that 91.29 kcal/m² is only 1.01 kcal greater than the 90.28 kcal/m² of the natural flora. This clearly raises the question of why do agriculture? The answer may lie in the relative expenditure of energy necessary to get the 90 kcal under different forms of subsistence or in the fact that not all of the floral productivity is humanly consumable. What it may indicate is that the change from gathering to agriculture is not a major quantum leap and thus brings into question Childe's concept of the agricultural revolution. However, much more testing of data world-wide is necessary before one could substantiate the above conclusively.

Stage 8

Stage 8 is an attempt to relate the modern environment to past environments. As Jim Schoenwetter (pers. comm.) has pointed out this is the most difficult and tenuous part of palynology. This is because it is impossible accurately to determine quantitatively the environment from the pollen rain. It is complicated due to six factors which Butzer (1964) points out. First, there is differential representation of pollen due to differing surface receptivity and differential preservation of pollen under different environments. Second, there may be over or under-representation of species due to small or excessive pollen production, insect pollination or easily decomposed pollen. Third, there is documentation of long distance transport of pollen by the wind. Distances sometimes exceed 100 km. Fourth, there is possible redeposition of pollen from older sediments. Fifth, pollen is transported by streams. Sixth, pollen sequences are often truncated or incomplete due to fire destruction of sections and interruptions or lateral distortion in the sedimentation process.

In order to determine quantitatively the previous biomasses from the present biomass two parameters need to be determined. First, one needs to find at what times in the past the biomass equalled that of the present. This gives a base line. Second, one needs to determine the amount of fluctuation around this base line. Several assumptions are necessary. First and most important is that if one has modern pollen rain being equivalent to past pollen rain, one assumes that the biomass at the two times are equal. Second, changes in external factors, such as climate, affect the microhabitats approximately equally.

Hevly has constructed pollen spectra from floors of sites in the Four Mile, Shumway, and Hay Hollow Wash archaeological areas arranged in chronological order (Hevly, 1964). His spectra show which areas of the spectra are most similar to the modern day environment. These periods are A.D. 275-350, A.D. 1100-1200, and A.D. 1350-1400. These are the base line figures on which will be mapped the modern environment for comparison.

In order to determine the amount of fluctuation multiple factors will have to be considered. Hevly (1964, pp. 113-114) concludes in his dissertation on the basis of a wide group of pollen spectra that:

The fluctuations do not appear to be random or significant variation of aboreal pollen but can be shown to be more or less synchronous over a wide area. Such changes may represent fluctuation of vegetation zones by as much as 500' suggesting that movement of zones similar to that documented historically in southern Arizona may have been occurring for many millenia.

This 500' factor happens to be the approximate difference in altitude between both microhabitats VII and III and microhabitat I. This difference then may be considered the maximum difference not for two microhabitats but for one microhabitat through time. It is now possible to put approximate limits on resource change through time. Using microhabitat I and microhabitat III, the change in standing crop is 44.1 g/m^2 and in productivity is -4.6 g/m^2 . If one uses microhabitat I and VII the change in carrying capacity over time is 8.4 g/m^2 and in productivity is -12.6 g/m^2 .

In other words, during the periods of time when the environment was moister and cooler than the present, one would not expect much more than a 44.1 g/m^2 increase in the standing crop of microhabitat III and a decrease in productivity of -4.6 g/m^2 . Similarly, one would expect for microhabitat VII a standing crop increase of 8.4 g/m^2 and a productivity decrease of 12.6 g/m^2 . In periods of time in which the environment was warmer and dryer than the present, one would have reason to expect the standing crop of microhabitat I to decrease by not more than

44.1 g/m² and productivity to increase by much more than 12.6 g/m². It should be noted that standing crop and productivity change inversely.

The author recognizes the number of difficulties and the gaps in the analysis. However, he humbly suggests that quantitative estimates of ecological parameters based upon observations are better than non-quantitative generalizations. At least it allows one to have an impression of the order of magnitude of the change one is considering.

RECAPITULATION

This study has attempted to examine the prehistoric environment of the Hay Hollow Valley from a quantitative perspective. It describes the environment, analyzes the microhabitats, and attempts to quantify the maximum parameters of change in the microhabitats. In addition it presents several methodological conclusions and innovations with regards to appropriate quadrat and transect size. First, the study shows that transects and even nested quadrats of traditional size are not sufficiently large to provide an accurate sample. Second, it shows that the six potential microhabitats are actually four floral microhabitats and six faunal microhabitats. Third, for each potential microhabitat standing crop figures (ranging from 71.9 to 24.6 g/m²) and productivity figures for the flora (ranging from 22.7 to 2.1 g/m²) are presented. Fourth, in no microhabitat does the faunal biomass equal more than 2.9 percent of the total biomass and averages only 1.5 percent of the total biomass. Between A.D. 300 and 1400 the data indicate that the fluctuation for particular habitats in the standing crop and productivity around the modern environment was not much more than 44.1 g/m² and 12.6 g/m². Finally, it should be noted that more sophisticated statistical analysis such as discriminant analysis, will probably refine or change these first results.

TABLE 6. Total animal transect data.

	Microhabitats						
<i>Animals</i>	I	II	III	IV	V	VII	VIII
<i>Mammals</i>							
Deer	2						
Antelope	1						1
Cottontail rabbit	3			2	6	1	
Jack rabbit				1	4	1	
Coyote						2	
Squirrel	2	1	1	1		1	6
Gray fox	1						

TABLE 6. (continued)

<i>Animals</i>	Microhabitats						
	I	II	III	IV	V	VII	VIII
<i>Reptiles</i>							
Lizard (collared)	1	1		2	2	1	
Lizard (striped)					3		
Snake						1	
Horn toad	1						4
Other lizards				2		1	
<i>Large Insects</i>							
Bug						1	
Fly	2			1			
Bee		7	2				
Grasshopper	11				2	5	
Butterfly						1	
Dragon fly						1	1
Cicala	1						
Snails	1						
<i>Birds</i>							
Hawk, night	7			1	1		
Buzzard, vulture	5	1		1		6	3
Raven	1			17		1	3
Crow	2		1			1	2
Jay	30	1		2			2
Dove	1			21	4	13	19
Says phoebe	2	1		13			6
Flycatcher	5	1		7		3	6
Mockingbird	17		1	3	8	23	14
Meadowlark	1			13		6	
Sparrow, vesper	1			47	1	4	75
Sparrow, brown	20	3		52	7	5	33
Barn swallow							1
Cliff swallow							4
Other hawks	1					2	
Red tail hawk			3			1	
Sparrow hawk							3
Towee	1						
Black-and-white warbler							3
Speedbird	1			1			1
Towns tanager	1			1			
Thrashers							4
Pewee				1			3
Blackbird				3			3
Owls							1
Orioles				3			
Plain titmouse					6		
Kingbird				3		1	
Other and unknown	26	5		25	11	10	49

TABLE 7. Total animal transect data by density per square mile.

<i>Animals</i>	Microhabitats						
	I	II	III	IV	V	VII	VIII
<i>Mammals</i>							
Deer	1.4						
Antelope	.7						.1
Cottontail rabbit	2.2			1.2	10.8	.3	
Jack rabbit				.6	7.2	.3	
Coyote						.6	
Squirrel	1.4	6.9	9.1	.6		.3	.9
Gray fox	.7						
<i>Reptiles</i>							
Lizard (collared)	.7	6.9		1.2	3.6	.3	
Lizard (striped)					5.4		
Snake						.3	
Horn Toad	.7						.6
Other lizards				1.2		.3	
<i>Large Insects</i>							
Bug						.3	
Fly	1.4			.6			
Bee		48.6	1.8				
Grasshopper	8.0				3.6	1.6	
Butterfly						.3	
Dragon fly						.3	.1
Cicala	.7						
Snails	.7						
<i>Birds</i>							
Hawk, night	5.1			.6	1.8		
Buzzard, vulture	3.6	6.9		.6		2.0	.4
Raven	.7			10.3		.3	.4
Crow	1.4		9.1			.3	.3
Jay	21.7	6.9		1.2			.3
Dove	.7			12.7	7.2	4.3	2.7
Says phoebe	1.4	6.9		7.8			.9
Flycatcher	3.6	6.9		4.2		1.0	.9
Mockingbird	12.3		9.1	1.8	14.4	7.5	2.0
Meadowlark	.7			7.8		2.0	
Sparrow, vesper	.7			28.5	1.8	1.3	10.9
Sparrow, brown	14.5	20.8		31.5	12.6	1.6	4.8
Barn swallow							.1
Cliff swallow							.6
Other hawks	.7					.6	
Red tail hawk			27.3			.3	
Sparrow hawk							.4
Towee	.7						
Black-and-white warbler							.4
Speedbird	.7			.6			.1

TABLE 7. (continued)

<i>Animals</i>	Microhabitats						
	I	II	III	IV	V	VII	VIII
Towns tanager	.7			.6			
Thrashers							.6
Peewee				.6			.4
Blackbird				.6			.4
Owls							.1
Orioles				1.8			
Plain titmouse					10.8		
Kingbird				1.8		.3	
Other and unknown	18.8	34.7		15.1	19.9	3.3	7.1

V

Defining Variability in Prehistoric Settlement Morphology

by

DAVID A. GREGORY

The rigorous study of prehistoric cultural systems depends in part upon the precise, problem-oriented definition of the phenomena under consideration. Within any class of data, the definition of a range of variability in those data with respect to a particular problem should be an initial step before further persuance of the problem. Archaeologists are employing increasingly precise and sophisticated methods for dealing with their data, and neglect of the careful determination and description of variability in archaeological phenomena may produce skewed results at best, and at the worst, perpetuated misconceptions.

The specific study discussed below deals with prehistoric settlement pattern. Chang's (1968, p. 3) definition of an archaeological settlement will serve us here:

... the physical locale or cluster of locales where the members of a community lived, ensured their subsistence, and pursued their social functions in a delineable time period.

Settlement pattern, then, is defined as the arrangement of these locales in space. More specifically, we are interested in what has been called the "morphology" of a settlement-subsistence system; that is,

... the kinds, quantities, and spatial configurations of material items that represent the skeleton of an extinct system for exploiting, processing, and storing food and other resources (Struever, 1968, p. 285).

The results of research designed to elucidate patterns of functional variability in the morphology of a particular prehistoric settlement system will be examined, followed by a discussion of some of the implications of this research for other work which has used settlement pattern data.

SINGLE ROOM SITES IN THE HAY HOLLOW VALLEY

The particular subsistence-settlement system which will be dealt with here is represented by archaeological remains from the Hay Hollow

Valley in east-central Arizona. These remains encompass a time period from about 2000 B.C. to 1350 A.D., and as in many other areas of the Southwest during this period, the development of an agriculturally based, sedentary population out of an essentially hunting and gathering one was witnessed. Several studies of various aspects of the development of this prehistoric system have used settlement pattern data of one sort or another in dealing with particular problems (e.g., Plog, 1969; Zubrow, 1971a).

During the summer of 1971, a study of single room sites in the Hay Hollow Valley was carried out. The impetus for this study came from two observations: (1) that previous archaeological investigations in other areas of the Southwest had uncovered single room structures (e.g., Wheat, 1954; Bradley, 1959; Skinner, 1965; Moore, 1971), but very little information concerning the function of these sites and their relationship to a larger settlement pattern was available; and (2) that the ethnographic literature provided sparse but temptingly suggestive evidence for the use of a variety of extra-village single room architectural units by Puebloan societies (cf. Moore, 1971). In most cases where single room sites are referred to in archaeological reports, they are simply included as briefly described incidentals in a larger excavation (e.g., Wheat, 1954), or they are assigned a conjectural function such as "farm shelters" or "granaries" (cf. Bradley, 1959), usually with recourse to ethnographic data. Given the kind of microenvironmental exploitation documented both ethnographically (Hack, 1942) and archaeologically (Woodbury, 1961) and the association in some cases of single room structures with these patterns of exploitation, it does seem logical that such structures may represent part of a settlement morphology of some antiquity in the arid Southwest.

A research strategy was designed to determine the function or functions of single room sites in the area and to lay the groundwork for relating these sites to the total settlement and subsistence pattern as it changed through time. The work proceeded under the rubric of multiple working hypotheses, with test implications designed initially to account for four functional possibilities: permanent occupation sites, hunting and/or gathering base camps, field houses, and shrines. The mutually exclusive sets of test implications constructed for each functional possibility were based on absolute differences in artifacts, relative frequencies of artifacts, site location, features, architecture, and palynological evidence. Ethnographic analogy was not used to construct the statement of expected patterning of material remains for each alternative, except in

the sense that the original four alternatives were particularly inspired by ethnographic data.

The criteria used for the inclusion of sites in the sampling universe was that of a solitary architectural unit of one room. A simple unstratified random sample was drawn from a total of 20 single room sites known from intensive survey of 17.2 sq. miles of the valley. While the sample could have been stratified temporally using dates based on ceramic collections, the accuracy of this technique is highly questionable in view of the small size of the collections from the sites. Four sites were completely excavated, and along with two previously excavated sites, form the 30 percent sample that is the basis for this discussion.

It became quickly evident that the sparsity or often total lack of artifacts would not produce significant results using the original hypotheses and test implications. This material is summarized below, followed by a brief discussion of selected aspects of the sites.

NS 684

15 Gray Indented Corrugated sherds; 1 core; pueblo type architecture with a firebox in the floor; floor area: 7.13 sq. m.

NS 702

5 Brown Indented Corrugated, Smudged Interior sherds; 1 Snowflake Black-on-White, Snowflake Variety sherd; pueblo type architecture with mealing bin in floor (coarse basalt metate and mano fragment in mealing bin); floor area: 3.9 sq. m.

NS 127

1 flake; 2 mano fragments; pueblo type architecture; floor area: 5.61 sq. m.

NS 156

"few" flakes; pueblo type architecture with firebox in floor; floor area: 5.5 sq. m.

NS 29

2 Red Indented Corrugated sherds; 5 Snowflake Black-on-White, Snowflake Variety sherds; 4 Show Low Black-on-Red sherds; 2 Show Low Black-on-Red, Corrugated Exterior sherds; 8 flakes; 2 mano fragments; basalt cobble architecture; floor area: 5.3 sq. m.

NS 69 (White, 1967)

30 Brown Indented Corrugated sherds; 24 flakes; 3 cores; basalt cobble architecture; floor area: 3.10 sq. m.

Two of the sites (NS 29, NS 127) are located on the alluvial flats of the Hay Hollow Wash, the primary drainage channel for the area. A third site (NS 69) overlooks these flats from a position on the side of the basalt capped mesa that forms the eastern boundary of the valley. Another site is situated on flats adjacent to a primary tributary of the main



FIG. 4. Floor of single room site, N.S. 702. Arrow (50 cm. long) points north; meter stick in background.

wash (NS 702), and the two remaining sites are located in juniper-pinon scrubland, one in the western part of the valley and about a mile from the main wash (NS 684), the other about a quarter of a mile to the west of the wash (NS 156). Assessing these locations in terms of the functional alternatives is difficult. All of the sites are on or near arable land, and NS 29 and NS 127 are proximal to what may be the remains of prehistoric canals. No pattern is discernable with respect to spatial clustering of either the sites in the sample or the 20 single room sites taken as a whole.

Two architectural modes are evident in these structures: One kind of architecture, represented by NS 29 and NS 69, consists of semi-circular or three sided "rings" of basalt cobbles. The rest of the sites in the sample have the general characteristics of puebloan architecture as it is found in the area; that is, are rectangular in shape and constructed of shaped sandstone blocks (fig. 4). Three or four courses of stone remained in all cases. Floors were discernable in all of the sites, but were not well preserved. The only evidence for any of the sites having been roofed over comes from NS 684 in the form of burned roofing clay. In the rooms of pueblo type architecture, only NS 156 showed solid evidence for an entrance in one of the walls.

The presence of a coarse basalt metate in a mealing bin in NS 702 may support the field house hypothesis and perhaps suggests that an initial processing of corn or other crops may have been performed in such units.

Dating of the sites in the sample would allow a much more precise assessment of their relationship to other sites of their time period and to the ecological conditions at that time. Unfortunately, no material suitable for radiocarbon dating was obtained in the excavations, and paly-nological analysis which will probably allow the dating of these sites has not yet been completed. The original ceramic collections indicate that none of the sites was used before A.D. 950, but this evidence is inconclusive.

Additionally, it should be noted that a recent and careful survey of the ethnographic literature concerning the use of single room units among pueblo groups suggests that the functional possibilities may be considerably greater than the four initially used in this study. Moore's (1971, p. 25) work indicates the following possible functions based on ethnographic data: children's playhouses, pinon ovens, hunting lodges, priest's huts, shrines, and field houses. Even these "most probably do not constitute a full set or complete list." The difficulty in incorporating this information into a research design, and, indeed, a primary problem in dealing with material from these sites lies in the necessity for defining limiting or mutually exclusive expectations for the patterning of material remains for each functional possibility (Moore, 1971, p. 24). This task proves difficult at the conceptual level and nearly impossible when working with the actual data. So whether or not ethnographic analogy is used in a study of single room sites, it will be difficult to determine specific functions for them. The pollen samples may shed some light on the problem, and an increased sample size would also aid in this effort.

In sum, we cannot at present subscribe to any of the functional alternatives originally put forth. It is possible, however, to eliminate one of the alternatives, and this elimination will be important to our discussion of variability in settlement morphology. We would contend that single room sites, regardless of the functional variability which may occur *with-in* this class of sites, do not represent permanent occupation units. First, the sheer lack of cultural material of all kinds indicates that these units were not being continuously used. Second, the absence of sets of features characteristic of the prehistoric subsistence mode as we know it would indicate that all of the activities required to sustain the population were not being carried out at these loci. Finally, it seems unlikely that even a minimal social unit could have successfully carried out their necessary

routine in the confines of the space represented by the floor areas of these sites, even allowing for the outdoor performance of many activities. Our conclusion is that single room units in this area are either functionally distinct or are limited and specific functional extensions of activities performed in permanent occupation sites. They do not, therefore, represent a permanent population locus.

While we have been unable to demonstrate and specify functional variability within the class "single room sites," there is a definite dichotomy in the morphology of the prehistoric settlement system of the Hay Hollow Valley: Some sites represent permanent occupation loci and some do not (single room sites and perhaps others). Let us review an example of how even this basic bit of information can produce significant skewing of results in problems using settlement pattern data.

Using the same survey material from which the sample of single room sites was drawn, attempts have been made to measure the relative distribution of population in the area as it changed through time (Gregory, 1969; Zubrow, 1971a). A nearest neighbor analysis (Evans and Clark, 1954; Haggett, 1966) was performed on the data, using temporal divisions of one hundred years based on dating by ceramic collections. The nearest neighbor analysis measures the relative distribution of a set of points in bounded space, and gives a coefficient ranging from zero to 2.15; Zero represents total aggregation of points, 1.0 represents a random distribution, and 2.15 is perfect hexagonal dispersion.

A basic and crucial assumption underlying the use of nearest neighbor analysis in measuring population distribution is that all the sites in the sample represent population loci; otherwise, we would have a measure of the distribution of *sites*, not population. In the initial measure, all surveyed sites were included in the sample, regardless of their size. Since we have shown that single room sites do not represent population loci, it is necessary to stratify them out of a sample which is proposed as indicative of population distribution.

As an example, the period of 1000 to 1100 A.D. will be used. The original nearest neighbor coefficient obtained for this period was 1.0369, or very close to random distribution (Gregory, 1969, p. 8). If the single room sites are stratified out of the sample and the nearest neighbor measure performed again, the resulting coefficient is 1.8112, representing a much more dispersed population.

The implications of this example are clear: If the problem is determining the distribution of population, then the relationship between sites and

population must be shown. In this case, we have seen that a sample of sites is internally variable with respect to this relationship, and that this internal variability is significant to the problem of determining population distribution. To obtain a more accurate measure of population distribution, those sites which do not represent population loci should be excluded from the sample. At a more general level, we may say that the definition of variability within a class of data with respect to a specific problem must be as precise as the information required by that problem.

CONCLUSIONS

The results of a study of single room sites in the Hay Hollow Valley indicate that determination of the specific functions of these sites is difficult due to (1) the lack of artifactual material forthcoming from the sites, and (2) the difficulty in determining mutually exclusive expectations for the patterning of material remains for each functional possibility. References to such structures in both ethnographic and archaeological literature suggest that single room structures may represent part of a settlement morphology of some antiquity. Further research is needed since knowledge of these sites will be important to a variety of problems using settlement pattern data.

Whatever internal variability may be represented in the class "single room sites," it has been determined that such sites probably did not represent permanent occupation loci, at least in the Hay Hollow Valley and perhaps in other areas of the Southwest. An example has been presented to show the skewing that can result from lack of precision in the determination and description of variability in archaeological phenomena.

ACKNOWLEDGEMENTS

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VI

The Joint Site — A Preliminary Report

by

JOHN A. HANSON AND MICHAEL B. SCHIFFER

INTRODUCTION

By the title of this paper, we have committed ourselves to presenting an account of the archaeological investigations in which we engaged during the summers of 1970 and 1971. What we present here is not to be considered a "site report" in any current usage of that term. This paper is a frankly experimental endeavor in which we have aimed one solution at the problems posed by the current uncertainty in the status (and prestige) of "site reports."

A survey of these reports suggests that their function and specific content is changing in the direction dictated by recent emphasis on problem-oriented archaeological research. Unfortunately, the specific trends and future directions that site reports are taking are not entirely clear. While some might suggest that data presentations are obsolete, we do not feel that this is the case—or that it is simply a question of data vs. problem presentation. In our opinion a site report must now serve two very basic functions.

The first of these is to provide in an accessible location the basic information which enables a reader to make sense of data applications concerned with the primary investigators' original problems and hypotheses. In other words, what one attempts to present is the lowest common denominator of site information which is taken for granted, or referred to, in both more specific and more general presentations and applications of the site data.

The second function that a site report must fulfill is to serve the community of archaeologists as a whole by providing the researcher with at least some of the information necessary to decide the site's potential for having data relevant to problems that interest him. Clearly, this kind

of information, to have its greatest benefit, should appear as quickly as possible after excavation. Otherwise it has only limited value in preventing the unnecessary excavation of sites or use of scarce research funds that can occur as a result of the duplication of effort. Obviously this is only an ideal; increasingly diverse research interests of archaeologists will often render gathered data useless for some problems. If an investigator decides that this already collected data would be of use to him, he can then invest time and energy in securing more details and access to unpublished data and/or the collections themselves.

We do not maintain that this is the only conceivable approach to defining the nature and function of site reports for the future. It is our argument that a discussion of alternative approaches to stating and resolving the site report problem is long overdue in the archaeological literature. Space limitations prevent us from achieving anything beyond this brief discussion of our ideas on the subject, illustrated by the following report on the Joint Site.

The Joint Site is a 36 room pueblo which was discovered by the New Survey of the Southwest Archaeological Expedition of Field Museum in 1968 (figs. 5, 6). Although not falling within any of the previously determined sample units, it was assigned the survey number NS 605 (for a more detailed discussion of the surveys of this area, see Zubrow, 1971a, p. 6). The Joint Site is located on the Carter Ranch, 10 miles east of Snowflake, Arizona at 34° 32' N lat. 109° 57' long. It is situated on a sandstone bench at an altitude of 5750 ± 25 ft. above sea level (pocket altimeter). It lies midway between the boundary of the Hay Hollow and Silver Creek drainages and the Hay Hollow Wash, 1½ miles west of Broken K Pueblo (Hill, 1970a; Martin, Hill *et al.*, 1967).

At the time of its discovery two important points were noted. The first was that only one pothole had been placed in the site (unusual in this area). Second, the site was late in time and of considerable size (survey estimate of 40 rooms) occurring on an upper sandstone terrace rather than the valley bottom (this clearly contradicts the predictions of Zubrow's (1971a) ecological model of population movement). A grab sample collection of surface pottery was taken. The sherds were inspected by Dr. Paul S. Martin who assigned the site a tentative date of A.D. 1000-1300 on that basis.

At the beginning of the 1970 field season we were faced with the need for locating a site which could meet the minimum specifications of three very different research designs while at the same time being accessible during the rainy months of July and August. The Joint Site fulfilled

these expectations to an optimum degree, given the constraints imposed upon our choice by the varying research designs. It must be conceded, however, that the site was a compromise for each of us now using its data. Two of the research designs are presented in detail elsewhere (in this volume), but a very brief summary of each will be presented here in order to illustrate why the Joint Site was deemed a useful location for conducting our experiment of using three research designs—the solution of several different problems on data from a single site.

Gorman was interested in the applications of the ethnoscientific approach to archaeological data. The research attempts a comparatively new method of artifact analysis to determine what material phenomena were significant for the prehistoric inhabitants of the Hay Hollow Valley and to discover how they cognitively organized these phenomena in different periods of time.

Analysis is focussing on the cognitive organizations of two levels of prehistoric culture in the Hay Hollow Valley. The Joint Site was the later of the two sites to be tested (the earlier site is a pithouse village dating A.D. 400-600). Lithics, ceramics, and architecture form the analytical or eliciting frames for the delineation of cultural items and features as remnants of prehistoric decision sets and their corresponding situational contexts. Variation in these patterns when derived together with the rules for their occurrence reflect prehistoric organizing principles.

Hanson is undertaking research seeking to measure the means by which occupants of the pueblo responded to environmental stresses which palynological work has indicated were getting increasingly severe during the pueblo's suggested lifespan. He is concerned with the effects of these stresses on intrasite social unit interaction. For this reason, a site larger than the Joint Site was considered, as he reasoned that in order to test hypotheses relating to this problem a site with the likelihood of at least two distinguishable social units larger than the nuclear or extended family was needed. Because the Joint Site was of the same general size as the Carter Ranch Site (see Longacre, 1970; Martin, Rinaldo *et al.*, 1964) and seemed to present similar possibilities for recovery of the necessary data pertaining to the problem, it was deemed satisfactory. It presented the opportunity to sample a considerable percentage of the habitation areas as well as the burial population (if such could be discovered).

Schiffer's basic interests concern the principles which underlie the cultural aspect of the processes responsible for forming the archaeological record. Specifically, several hypotheses devised to explain and

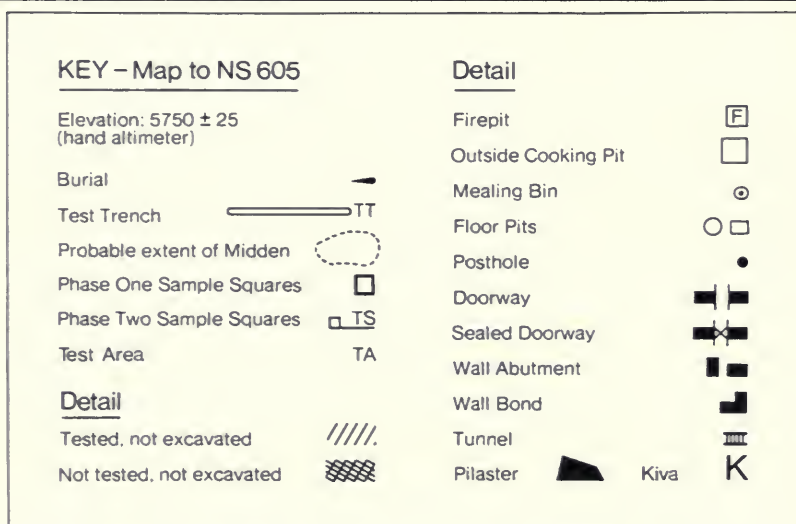
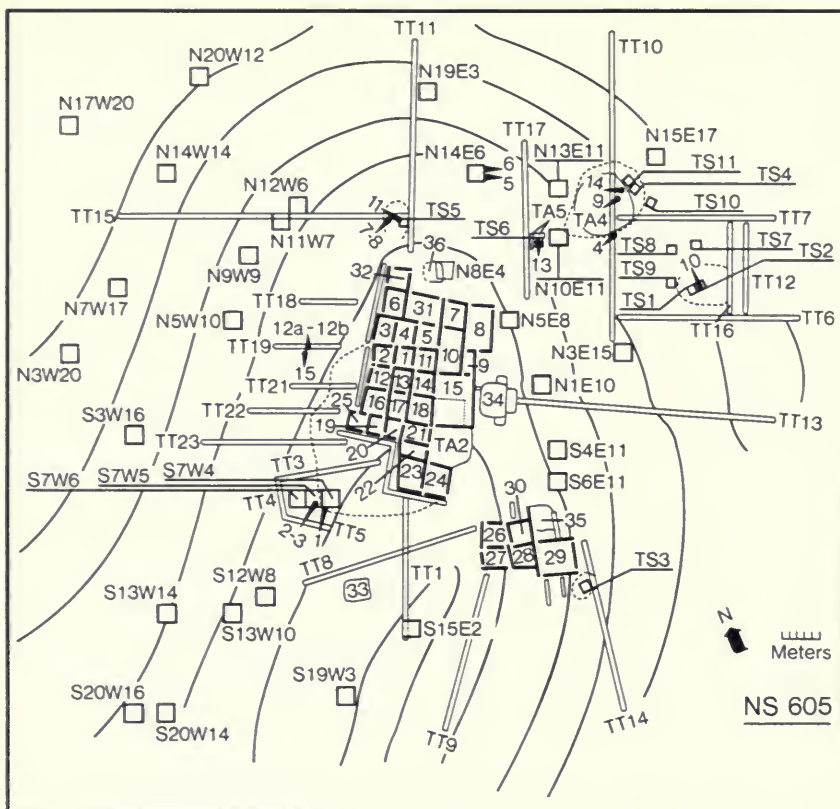


FIG. 5. Map of Joint Site Pueblo (N.S. 605), East Central Arizona.

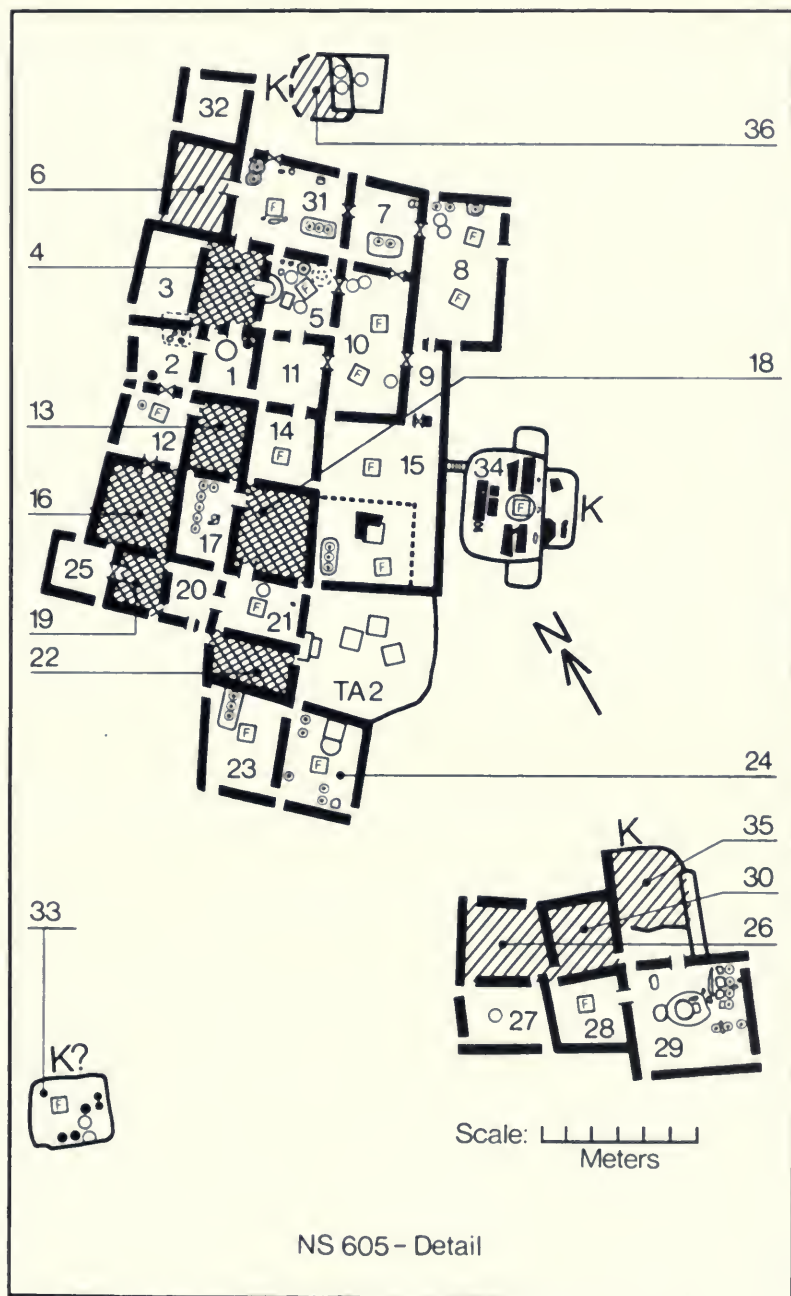


FIG. 6. Detail map of central room block of Joint Site Pueblo.

document differential refuse disposal patterns and the effects of site abandonment on *de facto* refuse content will be tested. Such testing will be facilitated by use of a flow model for cultural elements developed partially in an earlier paper (Schiffer, 1972).

In order to carry out these researches, Schiffer had to be able to state the total ratios of elements discarded at the site and compare them with ratios of different subpopulations of elements. These operations require a representative sample of both the architectural and non-architectural areas of the site, with particular attention, in later stages of analysis, to different secondary refuse locations.

In order to examine this set of problems, Schiffer required a site from which a representative sample of all artifact and feature populations could be acquired in two field seasons. A smaller site was originally thought to be preferable.

The Joint Site, before excavation, seemed to meet our specifications with minimum strain on the research designs, but also contained the potential for providing sets of data for other interesting problems. One of these is synchronic variability in social organization and patterns of subsistence adaptation within a region - but differentiated by ecological zone. These problems are feasible because of the data already available from the largely contemporary sites of Broken K and Carter Ranch (Martin, Rinaldo *et al.*, 1964; Martin, Hill *et al.*, 1967).

The major field operations and preliminary results will now be described as they relate to the sampling design for the site as a whole.

1970 FIELD SEASON

A. *Surface Collection*

An experimental design was utilized to recover a 36 percent sample of surface lithics and ceramics. The boundaries of the surface scatter suggested that the site was roughly square, about 80 m. on a side. We constructed a grid of 1 m. squares, 80 m. on a side which was then stratified into 16 equal squares measuring 20 m. by 20 m. The universe was then further stratified into two subpopulations. Eight of the large squares were selected randomly. Within each of these 20 m. by 20 m. squares, nine of the 25 4 m. squares were randomly selected. In the remaining eight large squares, two columns and two rows—each 2 m. wide—were randomly placed.

Before commencing the surface collections we believed that a one-third sample would be both representative and manageable to carry out.

In fact, it probably would have required about the same effort to take a complete surface collection using 4 m. by 4 m. squares. Although the two sub-populations of rows and columns samples (18 percent of total surface) and squares samples (also 18 percent of total surface) are not strictly comparable because of sampling error, we believe that the sample as a whole is representative of the total surface site since the initial stratification provided a guaranteed extensive areal coverage.

With the benefit of hindsight we can state that if our sole purpose in taking the surface collections had been to gain knowledge of the sub-surface site we could have done it much more efficiently by utilization of the backhoe. We do not now feel that the expenditure of so many man-hours of labor was justified by the meagre output in terms of predictability of the extent and variability of the sub-surface site. As it happens, however, the most important reason for making the surface collection was to be able to explain why and in what ways the surface is different from the subsurface of a site. Had we been able to afford extensive trenching and surface collecting in the first season, it is likely that the test square sample design (to be described below) would have been considerably different. Intensive excavation is best based on prior knowledge of variability in the subsurface site gained as quickly and efficiently as possible.

B. *Room Location*

After completion of the surface collection, we attempted to define the universe of rooms within the pueblo itself. This was accomplished by following visible walls to corners, and still more walls, until a reasonably accurate site map could be made. It was obvious at the outset that two distinct mounds exist at the site; an early objective of the wall location activities was to determine whether the two room blocks were indeed separate (fig. 7). They were. After the cornering project was completed the universe stood at 33 rooms.

Before an excavation sample was selected, we decided that the depth of the rooms would have to be known so that an estimate of excavation time could be made. Surface indications suggested that room 1 was likely to be the deepest on the site. We undertook the excavation of a 1 m. test trench within Room 1. It was soon apparent that the rooms were deeper than expected (some had 1½ m. of standing wall). We learned, in addition, that the excavation of preliminary stratigraphic trenches in rooms was generally precluded by the presence of a thick deposit of uncompacted aeolian sand which we later noted in nearly every room. Room 1



FIG. 7. Joint Site, northwest section with wall outlines exposed.

was completely excavated when a large quantity of floral material was discovered on the floor.

C. Initial Room Stratification

After completion of the preliminary site map, room areas were calculated and a scatter diagram of their distribution was made. Following the lead to site stratification provided by Hill (1967), we reasoned that rooms of different size housed different kinds of activities. Five classes were defined on the basis of the scatter diagram. Although this was a larger number of room classes than Hill obtained at Broken K, we preferred to overdiscriminate in the early stages of investigation; classes could be combined later at our convenience if the situation warranted it.

Rooms within each class were given an arbitrary number and, through the use of a random numbers table, given an excavation order. Although we knew that the entire site could not be excavated, this sampling procedure allowed the sample to be easily enlarged to any percentage within each class without resampling. The room classes and the room excavation order within them was as follows:

- A. (3.32 sq.m. to 4.84 sq.m.): * 1, 32, 2, 25, 4, 9, 33,
B. (6.36 sq.m. to 7.98 sq.m.): 11, 7, 21, 14, 17, 27, 3, 5,
C. (8.37 sq.m. to 9.88 sq.m.): 24, 23, 28, 18, 26,
D. (11.00 sq.m. to 17.85 sq.m.): 8, 29, 10, 6,
E. (17.86 sq.m. to 29.20 sq.m.): 15

*As all measurements were made before excavation, stratification does not include changes noted after excavation. Italics = excavated 1970.

Because room 1 had already been excavated, it was placed in the initial position within class A. Although this departs from the full randomness of the sampling design we do not believe that this inclusion introduced a systematic bias into the sample.

D. Room Excavation 1970

A total of 18 rooms was excavated from the initial stratified sample during the 1970 season. The above table indicates which rooms were excavated in each size class.

Near the end of the season an outlier was discovered after heavy rains exposed some of its very few courses of masonry. This room clearly fell into a class by itself because of its location, shape, and architecture (it was semi-subterranean) and we decided to begin its excavation. Approximately three-fourths of the room was dug. It was completed in 1971.

1971 SEASON

A. Redefinition of room universe

The first task we faced at the beginning of the 1971 field season was the relocation of room walls. With knowledge gained from the previous year's digging we decided that a distinct possibility existed that not all rooms had been discovered and mapped during the 1970 season. For this reason, a thorough cornering and mapping project was undertaken. During this project all wall joints were observed for bonding and abutment patterns. These data would provide a primary source of information for determining the construction sequence of the Joint Site pueblo.

This project resulted in several modifications of the site map. We discovered that two purported walls did not exist. This liberated two room numbers (30 and 33) which were assigned to the outlier (room 33) and a new room discovered in the smaller room block (room 30).

B. *Restratification of the sample*

The discovery of two additional rooms and the loss of two others meant that the original stratification, and hence sample, was based on faulty information. Assuming, however, that none of the mistakes introduced a systematic bias into our sample, we reasoned that it would be possible to take all known rooms, set up room classes as before, then random sample the unexcavated rooms within each class to obtain an excavation order. This restratification resulted in classes composed of the following rooms:

- A. (3.69 sq.m. to 5.46 sq.m.): (25), (32), (1), (2), 20, 9, 19,
 - B. (5.47 sq.m. to 7.33 sq.m.): (3), (17), (11), (21), (27), (28), (14), 12, 5, 13, 4, 22, 30, 16,
 - C. (9.34 sq.m. to 9.38 sq.m.): (33), (24), 6, 18,
 - D. (9.39 sq.m. to 13.22 sq.m.): (23), (7), 31, 26,
 - E. (13.23 sq.m. to 16.49 sq.m.): (10), (8), (29),
 - F. (16.50 sq.m. to 29.20 sq.m.): (15)
- Parentheses = excavated 1970
Italics = excavated 1971

This stratification was again based on room areas and was constructed from inspection of a scatter diagram. It was our hope to sample at least 50 percent of the rooms in each room block. Although this goal had been nominally reached following the 1970 excavations, we decided to increase the sample of rooms dug in some classes (particularly those with the greatest number of rooms). We felt that this would insure a representative sample both in terms of room classes and in areal coverage. We excavated rooms 20, 9, 12, 5, and 31 (see above) during the 1971 season. Although we had planned to excavate room 6, the discovery of three possible kivas led us to abandon the room after only partial excavation.

In addition to the fully excavated rooms, some testing was done in several others. The fill from rooms 26 and 30 was removed by the backhoe to a depth of approximately 10 cm. above the floor and collections from both rooms were made. Approximately two-thirds of the fill was removed from room 6 with the backhoe to a depth of approximately 20 cm. above the floor.

C. *The discovery of additional rooms*

In our investigation of the subsurface site, both with test squares and test trenches, three additional rooms, all semi-subterranean, were discovered. Two of these were certainly kivas, and a distinct possibility exists that the third was also. These were numbered in order of discovery but only room 34 was completely excavated.

Room 35 was discovered by means of a test trench. The fill was removed from this room with the backhoe and a collection of bone, ceramics, and lithics from the trash in it was made. A test trench (30 cm. wide) was excavated to floor along the entire length of the south wall of the room. A possible ventilator was discovered in the north wall.

Excavation of test square N8E4 revealed the existence of room 36. Owing to the pressures of time, only the section of the room which fell within the sample square was excavated. The south wall was traced to its meeting with the west wall by means of an exploratory trench. A ventilator was located in the east wall of room 36, within the test square.

ROOM EXCAVATION PROCEDURES

While rooms differed in the amount of fill material, height of standing walls, etc., the procedures utilized for excavation were basically similar throughout the site. After walls were completely outlined, the top layer of fill was removed. In most cases this consisted of wall and room fall, and was excavated, sometimes with help of the backhoe, without sifting, as an undifferentiated "fill" level. These layers were very often sterile except for occasional wornout grinding stones which had been recycled to serve in wall or roof construction. When densities of fill, i.e., amount of cultural material, increased the room was gridded into four sections. Some of the larger rooms, especially in classes E and F were excavated in six sections; rooms 9 and 28 were excavated in two sections. Because of the aeolian fill, it was most often not possible to excavate in natural levels. Rooms were usually excavated in arbitrary levels, varying from 10-20 cm. in depth according to artifact density; the higher the density, the tighter the provenience control. Field notes were kept of the changes in natural levels so that they could be compared with the arbitrary levels from each room. These levels were lettered beginning with A for each room from the first level encountered. Sections were given numerical designations. The materials from each level (excluding "fill") were screened ($\frac{1}{4}$ in. mesh) and treated as discrete proveniences.

A similar procedure was followed in subflooring. Beginning at the floor, arbitrary levels of 20 cm. were used until either the artifact density increased (at which point 10 cm. levels were employed), an occupation surface was encountered, or when sterile was reached and excavation was halted.

All features were given feature numbers and treated as separate provenience units wherever practical. Notes for each room were main-

tained by the excavation team (one to four people) in which were recorded all samples and their proveniences, floor and feature maps, profiles, and other basic information about the excavation of each room.

Pollen samples were collected from each vertical level and all floor sections. Additional pollen samples were taken from most features, including fire pits, mealing bins, ash pits, etc. In a number of cases flotation samples were taken from various ashey areas as well as other proveniences.

Because of the excellent organic preservation, many beams, both burned and unburned, were recovered. These have been sent to the Laboratory of Tree-Ring Research at the University of Arizona for analysis. Ten carbon 14 samples were submitted to Geochron Laboratories for analysis after the 1970 season. The results were generally disappointing. This dating procedure proved costly and of relatively little help *in our particular case*, because of the recycling of beams and the apparent collection and use of old beams in firepits.

Dendrochronological evidence suggests that much of the construction at the site occurred in the 1230's and 1240's (Wilcox, this volume). The date of the earliest rooms is unknown, but a good estimate seems to be 1175-1200. The time of site abandonment has yet to be fixed, but a non-cutting date of A.D. 1255 has been obtained from room 31.

ROOM EXCAVATION: PRELIMINARY RESULTS

A. *The Sample*

It has been mentioned that one reason for the construction of our particular sampling design was to provide a representative sample of room classes. We felt that in all cases 50 percent was the minimum acceptable sample. Percentages taken in the various classes at the close of the 1971 (and final) field season are as follows:

SIZE CLASS

- A—5/7, 71.4%
- B—9/14, 64.3%
- C—2/4, 50%
- D—3/4, 75%
- E—3/3, 100%
- F—1/1, 100%

AREAL CLASSES

- LARGE Room Block—20/27, 74%
- SMALL Room Block—3/6, 50%

ARCHITECTURAL AREAS AS A WHOLE (Both room blocks, outlier and kivas)
—25/26, 69.4%

This does not include partial excavations which are described elsewhere.

B. *Room profiles*

In order for data from the Joint Site to be useful for our problems, and hopefully those of other investigators, a minimum understanding of site history is necessary. In the following section we present some basic data and preliminary interpretations concerning changes in room function. Of particular importance to our projected analyses is knowledge of which rooms were in use at the time of site abandonment, as well as those used for dumping refuse. Eventually, we hope to provide an abandonment sequence in addition to the construction sequence presented elsewhere in this volume by Wilcox.

Included in the following brief room descriptions are size class, area, and a description of hypothesized room function with evidence for our conclusions. Within the parentheses following each room number is the following information in order of appearance: room size class and exact floor area (to inside corners).

It should be underscored here that what follows are preliminary findings. Many of the statistical analyses of room material are only beginning at the time this is written. Therefore these interpretations *may* be subject to *some* change in future publications.

Room 1 (A, 4.87 m²)

This room was built on an earlier exterior occupation surface which contained one large circular pit as well as several disconformities in the surface and a small amount of refuse. In association with the room floor was a small firepit which had been plastered over sometime during the occupation of the room. We believe the room may have been a limited function habitation room, or habitation room initially and a storage area in secondary use. The final use of the room was for dumping. Preliminary analysis of charred floral material recovered from the dump has documented the presence of at least 18 species among over 12,000 identifiable fragments (Richard Hevly, pers. comm.).

Room 2 (A, 5.46 m²)

This room was also located on a prior, exterior occupation surface. A pit similar to the one found in the subfloor of room 1 was detected under the north wall. A comparatively small size and the absence of floor fea-



FIG. 8. Joint Site: Room 2, west wall; doorway leads to room 1. Arrow (30 cm. long) points north; meter stick in background.

tures suggests storage as the principal room use. Some of the material dumped into room 1 spilled through an open doorway into room 2 (fig. 8). This suggests that room 2 was abandoned either earlier than or at the same time as room 1. It does not seem likely that storage activities would be conducted in it while it was connected openly to a room being used as a refuse area for organic materials.

ROOM 3 (B, 6.08 m²)

Size, the absence of floor features, and the relative sterility of the floor and fill levels suggest that room 3 was a storage room, being used prior to site abandonment. As with rooms 1 and 2, room 3 was also built on a prior exterior occupation surface. This was indicated primarily by the fact that the subfloor pit discovered in room 2 continues under the south wall of room 3 into the room.

ROOM 4 (B, 6.42 m²)

Room 4 was not excavated, but exploratory soundings through an open doorway revealed that discard activities had taken place. This suggests that

the room was probably not in use near the time of abandonment.

ROOM 5 (B, 6.84 m²)

Early uses of the floor of this room included storage and food preparation activities. Evidence for the former is in the form of a large bell-shaped pit similar to ones found in room 12 and at the Carter Ranch Site (Martin, Rinaldo *et al.*, 1964). Food preparation activities were evidenced by the presence of mealing bins. At some point the function of the room apparently changed. A large central firepit was put in and the bell-shaped pit was filled with ash and other refuse. This firepit was then floored over suggesting a further change, possibly to storage activities. The room was abandoned and used for dumping, which in some areas left a deposition of 80 cm.

ROOM 6 (C, 8.46 m²)

This room was only partially excavated. Size and shape of the room would tend to support a habitation and/or food preparation - storage



FIG. 9. Joint Site: Room 7, east wall; mealing (?) pit in foreground; blocked doorway to room 8. Arrow (50 cm. long) points north; meter stick in background.

interpretation. The room may have been occupied until just prior to site abandonment since no dumping material was found in the fill. It must be conceded, however, that we never reached floor which may have been as much as 20 cm. below the point where excavation stopped.

Room 7 (D, 11.05 m²)

Although a relatively large room, the absence of any firepit or other notable cooking area suggests that the room was not a habitation area. In floor association were numerous metates and grinding stones as well as mealing bins. This suggests that room 7 probably was a center for food-preparation activities. Blocked doorways separated room 7 from room 8 and room 10. These doorways were partially blocked-up with worn metates (fig. 9). Room 7 was apparently not occupied at site abandonment, as discard activities were evident in the fill.

Room 8 (D, 16.30 m²)

Room 8 stands out from most other rooms on the basis of several features, notably size, shape, location, and the possession of two floor



FIG. 10. Joint Site: Room 8, floor, note the two firepits. Arrow (50 cm. long) points to north; meter stick in background.



FIG. 11. Joint Site: Room 9, floor. Arrow (50 cm. long) points north; meter stick in background.

firepits (fig. 10). With rooms 10 and 15 (and possibly 31) it seems to comprise a distinctive functional class. We hypothesize that these late rooms were habitation areas for residential groups larger than a nuclear family. The two firepits would seem to offer some material evidence for this interpretation. There is no good evidence for change in function during the room's occupation and the absence of dumping (=discard) activities suggests that it was not abandoned before the site as a whole.

Room 9 (A, 3.87 m²)

Room 9 was almost certainly used for storage as its size alone would have precluded living in it. Tree-ring dates suggest that it was constructed in the late 1230's. There were no floor features and only very little cultural material in the fill. Room 9 was probably still in use just prior to site abandonment. An open doorway led to room 8 (fig. 11). Blocked doorways separated room 9 from rooms 10 and 15. In the subfloor of this room two distinct "plaza" surfaces were discovered. At the end of the 1971 field season work had progressed to a depth of 1.40 m. below the base of the room walls but sterile soil had not yet been reached. At the 1.20 m. level below floor we discovered a subterranean structure. Unfortunately, time prevented us from ascertaining positively whether it was a kiva, pithouse, or something else. Room 9 burned hot enough during its occupation to fire the adobe mortar and plaster to a buff-pink color. It is likely that this burning took place prior to room abandonment as no evidence of charcoal or other fuel for such a fire was found in the room.

Room 10 (E, 14.66 m²)

As with rooms 8 and 15, room 10 is large, long and rectangular in shape, and is situated with the long axis on a basically north-south orientation (fig. 12). It also has two firepits in the floor. In the room fill, five unburned beams from the collapsed roof were found lying against the south wall. We believe that room 10 functioned very much like room 8. Although room 10 was not subfloored completely, it appears that it did not change function during its occupation. It was apparently built over older occupation surfaces and likely the same subfloor structure which underlies room 9. Room 10 was one of the last rooms abandoned.

Room 11 (B, 6.70 m²)

Room 11 appears to have been built on an exterior occupation surface which contained no features or other items of interest. The room itself was almost certainly built to serve as a storage facility. Evidence for this is indicated by a lack of floor features and small size. After abandonment



FIG. 12. Joint Site: Room 10, east wall. Arrow (50 cm. long) points north; meter stick in background.

of the room it was used as a dump and a deposit of 20 cm. provided a wealth of cultural material.

ROOM 12 (B, 6.72 m²)

Room 12 provides an excellent example of how the function of a room may change during its occupation. Indications from the second floor (earliest) in the form of mealing bins, grinding stones, and a large bell-shaped storage pit suggest that the room was originally constructed to serve as a center for food preparation and storage activities. The floor surface was then completely plastered over and a firepit was placed in the upper floor surface. At the time of this change it would appear that a habitation room was needed more than a food preparation-storage area. This is one of the few examples of obvious reflooring present at the site (figs. 13 and 14). It also shows an interesting transition from one kind of task-specific area to another. Room 12 was probably one of the last rooms abandoned.

ROOM 13 (B, 6.23 m²)

Room 13 was neither excavated nor tested.

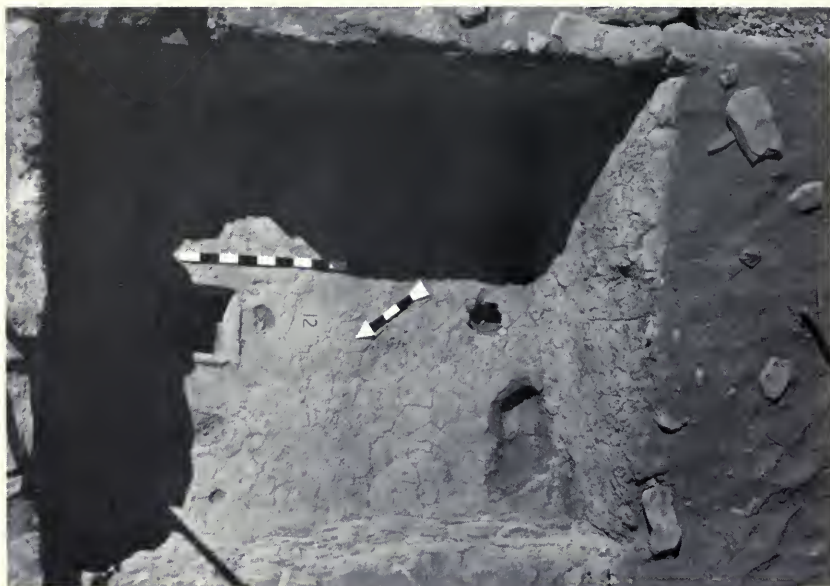


FIG. 13. Joint Site: Room 12, floor 1. Arrow (50 cm. long) points north; meter stick in background.

ROOM 14 (B, 7.23 m²)

Room 14 had a firepit in the floor suggesting that it may have functioned as a habitation area. Its small size, general lack of other features, and few artifacts of primary or *de facto* refuse (Schiffer, 1972) might support an interpretation of limited habitation-storage activities. The room was apparently one of the last abandoned, and must, therefore, have had a long life, since it was constructed in the core unit (Wilcox, this volume).

ROOM 15 (F, 29.20 m²)

Room 15 is a special room in many ways. It shares basic shape, long axis orientation, and general size with rooms 8 and 10, although it is larger than both put together. The room may have been built in two construction episodes. This is not entirely clear. Like rooms 8 and 10, room 15 also had two firepits. The room was used for various food preparation activities, indicated by mealing bins, butchering tools, and bone, as well as for probable habitation (fig. 15). Room 15 also served as an entry-way to the large D-shaped kiva, located directly to the east. Entry was made possible by means of a tunnel which opened in the floor under the north half of the eastern wall of room 15.



FIG. 14. Joint Site: Room 12, floor 2. Arrow (50 cm. long) points north; meter stick in background.

Sometime during the occupation of the room a pilaster was built in the southern half (fig. 16) to relieve the walls of some of the weight of the roof (the builders of the Joint Site were notoriously poor architects). Evidence of wall repairs suggests that wall collapse may have occurred several times.

Room 15 was subfloored only in the southern half. The rectangular floor of a small room of undetermined function was discovered under some refuse at a depth of approximately 15 cm. below the floor surface. It contained a firepit but no other features.

We feel that all indications point to Room 15 as being one of the last abandoned.

ROOM 16 (B, 7.23 m²)

This room was neither excavated nor tested.

ROOM 17 (B, 6.54 m²)

Room 17 was built on sterile native soil. The room's original function seems to have been as a food-preparation-storage center, judging from the line of mealing bins which ran down the center of the floor. There was no firepit. Room 17 was abandoned before the end of site occupation and converted into a dump, which, in the northern sections of the room, reached a depth of 80 cm. Excavation of this dump provided us with a considerable amount of cultural material (over 3,000 sherds alone).

ROOM 18 (C, 8.26 m²)

No tests or excavations were carried out in this room.

ROOM 19 (A, 3.75 m²)

No tests or excavations were carried out in this room.

ROOM 20 (A, 3.89 m²)

There were no floor features in room 20 and relatively little cultural material in any provenience. These observations, added to its small size,



FIG. 15. Joint Site: Room 15, floor. One-meter tapes on the floor in center and rear of the room.



FIG. 16. Joint Site: Room 15, "Pilaster"; the outer plaster layer has been removed. Arrow (50 cm. long) points north; meter stick in background.

suggest a function of storage. The room was most likely one of the last abandoned.

ROOM 21 (B, 7.12 m²)

The central firepit in the floor and some primary and *de facto* refuse suggest that this was a habitation area. Some food preparation activities also likely took place, either in the room or on its roof. This is suggested by the number of manos and metates lying on the floor or just above the floor. At this point, we feel that room 21 was being used near the end of site occupation. This conclusion may be subject to change, however, as there

was some spillage of material from an open niche into room 18. We have not as yet determined for certain whether this occurred prior to site abandonment or after it.

ROOM 22 (B, 6.69 m²)

This room was neither excavated nor tested.



FIG. 17. Joint Site: Room 23, floor, with floor and wall slabs in place. Arrow (30 cm. long) points north; meter stick in background.

ROOM 23 (D, 10.20 m²)

Like many of the other late rooms, room 23 appears to have been multifunctional. It apparently was used as a habitation room, considering its larger than average size and the presence of a firepit (fig. 17). The presence of mealing bins and manos in one corner suggests that food preparation activities were carried out. The possible importance of the roof as an activity area is suggested by the presence of two large "griddles," large flat slabs of worked stone. These also occurred in rooms 8, 9, 10, and 21. Room 23 was one of the last rooms abandoned.

ROOM 24 (C, 9.88 m²)

Room 24 appears to have been a habitation room in which a variety of maintenance activities were performed (fig. 18). A firepit and mealing bins were present. A large roasting pit which runs under the north wall of the room suggests that the construction of room 24 encroached upon a formerly more extensive plaza area. Many other pits were



FIG. 18. Joint Site: Room 24, floor; stone features in place. Arrow (50 cm. long) points north; meter stick in background.

found in room 24 but the direct association of many with either the earlier plaza surface or floor cannot be established with certainty because a heavy rain during excavation turned the floor into mud. Some were almost definitely associated with the floor because they had stone slab covers. This room was also one of the last abandoned.

ROOM 25 (A, 3.32 m²)

Room 25 was almost certainly a center for storage. There was a recognizable dearth of artifactual material in the fill and no real floor features. Room 25 also has the smallest floor area of any room on the site. As with room 9, a very high intensity fire had given the plaster on the walls a near ceramic consistency. Burned beams in the fill suggest that this fire occurred after the room had been abandoned. This would most

probably place this fire at site abandonment also, as it would appear that room 25 was being used just prior to the end of site occupation.

ROOM 26 (D, 10.95 m²)

The fill from this room was removed by backhoe and a collection was made. A firepit was located near the center of the floor, indicating habitation as one possible function of the room. No further excavation was conducted.

ROOM 27 (B, 7.11 m²)

There was little material in the fill of this room (which was quite shallow) and relatively few artifacts on the floor. Only one small floor-pit was found. The virtual absence of features coupled with relatively small size (for this room block) suggests that this room functioned as a storage center. Room 27 was among the last rooms abandoned in this room block.

ROOM 28 (B, 7.23 m²)

This room was apparently a habitation room judging from floor associated material and features. There is a central firebox with some manos and metates lying nearby. It would appear that this room was one of the last abandoned.

ROOM 29 (E, 16.49 m²)

Room 29 is the second largest room in the site (excluding the large kiva). It was clearly a multi-functional room. There were rows of mealing bins on two sides as well as many pits mostly in the center of the room suggesting that numerous food preparation and storage activities were carried out (fig. 19). Curiously, there was no firepit. In many ways this room reminds the investigators of rooms 8, 9, 10, 15, 23, and 31. All are large and multi-functional, although in the case of room 29, habitation was apparently not a function, which was clearly the case in the other rooms mentioned. This point will be taken up elsewhere (see Hanson, this volume), in which changes during the growth cycle of the pueblo are considered in greater detail.

ROOM 30 (B, 6.77 m²)

The fill of room 30 was excavated by the backhoe and a collection



FIG. 19. Joint Site: Room 29, floor: note mealing bins in northeast, pits in middle. Arrow (50 cm. long) points north; meter stick in background.



FIG. 20. Joint Site: Room 31, floor. Arrow (50 cm. long) points north, meter stick in background.



FIG. 21. Joint Site: Room 34 (Kiva) floor (with firepits). Arrow (50 cm. long) points north; meter stick in background.

was made. The room may have been used for some discard activities. It did not contain a firepit. No further excavation was conducted.

ROOM 31 (D, 13.22 m²)

This room was multi-functional to judge by the abundant *de facto* refuse and floor features. There were numerous mealing bins as well as many manos and metates (fig. 20). Near the center of the room a large unslabbed firepit had been cut into by a later slabbed firebox. There was a great deal of broken pottery on the floor, some of which may have been resting on the roof or interior shelves. This room was clearly one of the last to be abandoned. Subfloor excavation uncovered *no* features, sterile soil being reached within 20 cm. of the floor surface.

ROOM 32, (A, 4.36 m²)

Room 32 presented us with direct evidence of its function. In it were found stacks of charred corn still standing against the wall of the room, and resting on a reed mat. Samples of this corn were taken for further analysis. Room 32 may have been intentionally burned by the occupants as they departed.



FIG. 22. Joint Site: Room 34 (Kiva), west wall. Meter stick in background.



FIG. 23. Joint Site: Room 34 (Kiva), east wall. Meter stick in background.

ROOM 33 (C, 8.79 m²)

Room 33 was discovered and excavation begun in 1970. Excavation was completed in 1971. It is located south and west of the two room blocks but not connected to either (although visible to both). The room is semi-subterranean, having been dug approximately 1 m. into sterile caliche. The walls were topped by several courses of masonry and the roof presumably rested on these. Its function is unclear at this time but the style of architecture and location suggest that it may have served as a ceremonial area (for whom is at present uncertain). What we believe to have been a ventilator was found on the south wall. There was a central fire-box and a "sipapu" (possibly a post-hole). There was no bench. Both floor and fill levels contained few artifacts. It is our belief that this structure was either not completed or had ceased as an activity locus sometime prior to site abandonment.

ROOM 34

Room 34 appears to have been the main ceremonial center for the inhabitants of the village at the end of site occupation. As can be seen in Figure 6 and the photos (figs. 21, 22, 23) this was a D-shaped, virtually subterranean, tunnel entrance structure. It was discovered and excavated during the 1971 field season. It has a rectangular floor area of 10.69m², with benches on the north, east, and west sides. A tunnel entrance in the west wall led to room 15. This structure, which can be safely called a kiva, had been built by excavating approximately 3 m. into the sterile caliche (which first-hand experience suggests is no mean feat). Masonry pilasters had been constructed in the four corners. A series of 38 tree ring dates suggests that this kiva was constructed in A.D. 1247. This was also a period which saw the construction of many other rooms in the northern block. It is not clear at present what the significance of this building period is.

There is little doubt that this kiva was being utilized at the time of site abandonment. Charred roof beams were in direct contact with the floor. There was comparatively little material in the approximately 35 cubic m. of excavated sand fill. The suggestion is that no habitational refuse disposal took place subsequent to the room's abandonment.

Little in the way of artifactual material was recovered from room 34. There was a ventilator which opened to the east, a slab fire-box, an ash pit, and deflector in the east-central portion of the floor. The floor was partially paved with flag-stones, in a pattern reminiscent of a bird-like design. The eastern bench may have served as an altar, as it was partially covered with flagstones; the north and south benches were plastered with

adobe. A mortar and pestle and a broken sandstone effigy (bear?) completed the cultural inventory on the floor surface. A subfloor pit under one of the flagstones was sterile. This evidence suggests that this structure was intentionally burned when the site was abandoned. Parenthetically, it should be noted that the tunnel entrance is a real oddity in the Hay Hollow area, although such entrances are quite common in the Chaco and Mesa Verde areas.

ROOM 35

Little which is substantial can be said about room 35. It is in the smaller room block and was discovered in the next to last week of the 1971 season. It stands out from other rooms in this unit in several respects. Most striking is that the floor is about 1 m. below the floors of adjacent rooms. Secondly, the room was excavated by its builders into the native caliche and only the west and south walls were masonry to the floor. The two remaining walls were natural caliche topped with courses of masonry. Thirdly, many discard activities had taken place in this room, from which collections of ceramics, lithics, and bone were made. The discovery of a ventilator in the north wall coupled with the other evidence led us to the tentative conclusion that room 35 had functioned as a special structure, perhaps for ceremonial activities. It had been abandoned some time before the end of the site's occupation.

ROOM 36

This room was discovered during the last week of the 1971 season while digging test square N8E4. It appears to have been a ceremonial structure used for dumping activities after its original use was discontinued. The structure was semi-subterranean and a ventilator was found on the east side. The section of this room falling within N8E4 was excavated to floor.

BURIALS

Sixteen burials were excavated at the Joint Site during the 1971 field season (see table 8). The number, though not large, is significant in that the excavation of burials in the Hay Hollow Valley had previously been confined almost entirely to the Carter Ranch Site (see Martin, Rinaldo *et al.*, 1964; Longacre, 1970). The burial population is heterogeneous in terms of placement. Burials were discovered in three basically different areas of the site; in the southwest, north-northwest, and northeast.

Burials were located in many ways. Most often they were discovered during the excavation of test squares and/or test trenches. When a burial was encountered the excavation team carried through a number of steps.

The first of these was to define the burial pit. This was oft-times very difficult due to the general practice of placing bodies in midden deposits. While this led to some arbitrariness in the delineation of pits we do not believe that this adversely affected either excavation or analysis. When the pit had been delineated the bones and grave goods (if any) were uncovered. Careful attention was paid to any changes in natural stratigraphy and the general depositional sequence. Once the grave was fully uncovered the body was sketched on the burial forms as was a cross-section of the burial pit matrix. Photographs were taken of all burials.

Pollen samples were collected from three areas of each body (when-ever possible): the cranium, the pelvis, and the feet. When ceramic grave goods were present, pollen samples were extracted from them. Burials were sexed and aged in the field when this was possible. Each burial was then tied into the overall site grid by means of the sample square grid in which the burial pit was located.

We thank the staff of the University of Arizona Field School at Grass-hopper for making their burial forms available to us.

TEST SQUARES

A. Stratification of Phase 1

An important part of the research design involved the necessity of controlling for various locations of activity performance and refuse discard, whether these fell within or beyond the pueblo walls. In addition to the extensive room excavation, we decided to design a sample of test squares to be located within spatial strata defined on the basis of the density of artifacts recovered in the surface sample of 1970. Although the relationships between the surface and subsurface site was one of the problems we were to investigate, it seemed worthwhile to make use of the surface information for designing a stratified sample. We are making the assumption that in some way use of surface density gradients as a basis for stratification will provide a better sample than a simple random sample, or systematic sample, or stratified systematic unaligned sample (Haggett, 1966).

A map was constructed of all surface provenience units and the density of lithics and ceramics for each was recorded. It was observed that there

was a close correlation between ceramic counts and lithic counts for all surface provenience units ($r = .91$).

It was apparent from the distribution map that obvious density differentials existed over the site. In order to illustrate this more clearly and enable strata to be defined, the map was modified so that different colors represented different density intervals for both the lithics and ceramics of all units. Artifact density gradients showed up remarkably well using this simple method. From the modified map major areas of uniform artifact density were identified and a coarse-grained stratification emerged. Because it was known beforehand that sample size would be necessarily small owing to labor priorities during the field season, an overly detailed stratification was not desired. Such a stratification would have resulted in numerous problems of disproportionate sample percentages among the strata. A 2 percent sample of non-architectural areas of the site was projected to be all that could reasonably be accomplished in about three-fourths of the field season, leaving sufficient time for the excavation of phase 2 test squares.

The next task at hand was the selection of the excavation unit size. Several important variables influenced this decision. One of the functions of test squares was to locate burials. This made it necessary to use as large an excavation unit as practical if we were to have a greater chance of discovering and identifying features (see Treganza and Cook, 1948). The second consideration was the difficulty of excavating small squares to a great depth. We knew from information gathered by test trench 1 that more than likely some sample areas had a deposit greater than 1 m. in depth. None of us joyfully contemplated excavating a 1 m. square pit to a depth of at least a meter. Clearly the ease of excavation factor argued for the use of larger sample units.

Weighing heavily against considerations for large sample units was the need to sample as many areas on the site as possible. This would help ensure a representative sample of artifacts from the site as a whole. Greater sample coverage for artifacts (as opposed to features) favored the use of small excavation units. A compromise size of 2 m. by 2 m. squares was finally used for phase 1 of the sample. This once again illustrates the practical difficulties of reconciling excavation procedures to different research designs. Had we been seeking only burials and features, larger excavation units would have been used. Had we been seeking artifact samples only, smaller units would have been used. Obviously, if more labor and/or time had been available, the compromise would have been unnecessary.

Phase 1 test squares—excavation procedures

All sample squares were excavated in arbitrary 15 cm. levels until sterile soil was definitely reached—at which point excavation was halted. In most cases natural stratigraphy was followed where this was possible; especially when a plaza surface, burial pit, or other feature was encountered. In most cases changes in natural stratigraphy were recorded in the field notes.

Proveniences were also controlled to the quarter of the square. Quadrant sections were employed as they were in room excavations. Proveniences were designated in the same manner; a letter for the level and a number for the section. Although it would have been preferable to follow natural stratigraphy in all cases where it existed, this extra effort could not be justified either in terms of research interests or our allocatable labor.

All material from all sample squares was sifted through a 1/4-in. mesh screen. Pollen samples were taken from one section of each vertical level with multiple samples being taken from important sections (see section on burial excavation procedures).

Five of the sample squares were not excavated after it was determined in preliminary testing that they contained no subsurface site. Surface material was located directly on native cobbles, which, in other areas of the site were always 5-10 cm. into the sterile original surface. Another square was excavated only partially; however, the deepest excavated level contained very few artifacts and was quickly going completely sterile.

Phase 2 test squares

The purpose of the phase 2 sample squares was to intensively investigate small portions of midden areas discovered by test trenches. No rigorous sampling method was used to locate these test pits. Often they had to be placed between mounds of back dirt or previously excavated pits or trenches (phase 2 squares were excavated during the final two weeks of the 1971 season). As in phase 1 excavation was by 15 cm. levels unless marked natural stratigraphy or features were encountered. As expected, many of these pits revealed burials.

Test Trenches

During the course of the 1971 season 27 test trenches were excavated with the backhoe. There were several purposes for putting in this network

of trenches: 1) to define precisely the extent and variability of the subsurface site, 2) to locate features, especially burials and midden areas not discovered by phase I sample squares, and 3) to locate possible undiscovered rooms. These trenches turned out to be remarkably informative. Rooms 34 and 35 were located in this manner and probably would not have been located in any other way as there were absolutely no surface indications of their presence. In addition to the two rooms, the test trenches uncovered six burials and ultimately led to the discovery of three more. Several midden areas, important to our research designs and not sampled by phase I test squares were also encountered. For nearly all test trenches, collections were made as dirt was dumped from the backhoe bucket. Such collections vary in comparability, as in some cases only ceramics were collected, while in others collections of ceramics, lithics, and bone were made.

We would recommend the use of an extensive network of backhoe test trenches to other archaeologists investigating sites generally similar to the Joint Site. We make this qualification, as a system of trenches such as we utilized would not necessarily be feasible in sites of other size or functional classes. It is our opinion that in this case, however, the test trenches and the use of the backhoe to dig them were invaluable in our assessment of the complexity of the Joint Site and our efforts to correlate surface and subsurface materials.

Test Areas

In some special cases non-architectural areas of the site were designated for broad excavation. The size of the various areas differed depending on context. In most cases these test areas were either plaza surfaces or midden areas that we hypothesized would contain burial materials (fig. 5).

PRELIMINARY RESULTS OF TEST SQUARES AND TEST TRENCHES

For purposes of this discussion, the terms *surface site* and *sub-surface site* will be defined. The surface site consists of all artifacts, features, and other properties of a site that can be observed (not inferred) without benefit of excavation. The subsurface site is composed of all archaeological remains below the surface. It should be clear that the subsurface site does not refer to the way the site looked at the time of abandonment. The subsurface site is simply what is below the surface at the time the archaeologist undertakes his investigation.

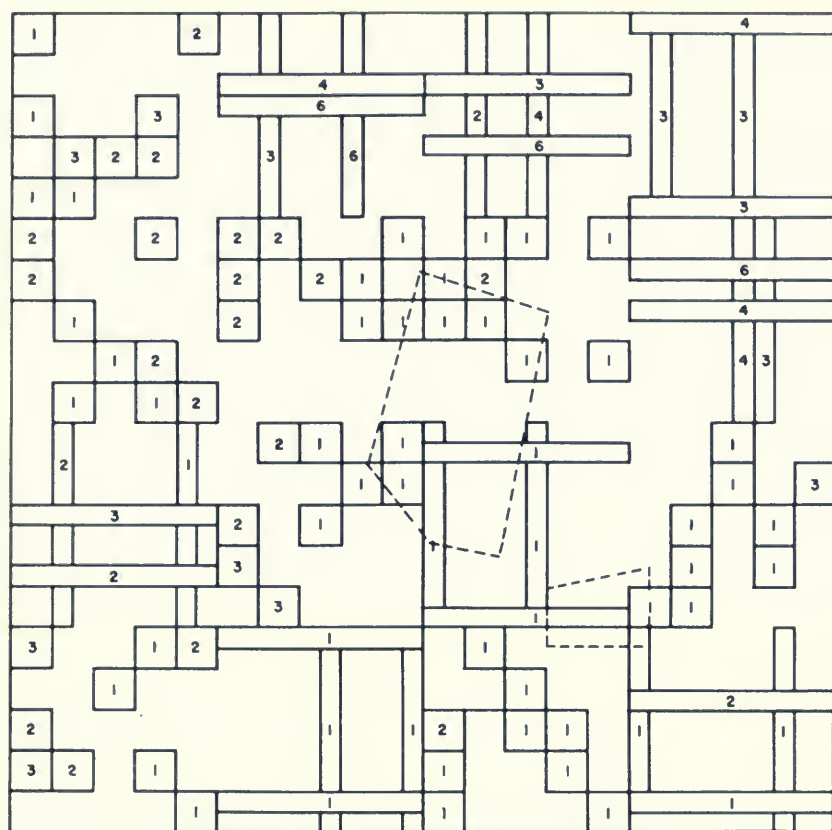
The most notable, and obvious, result of the sample square and test trench-test area excavations was the information that the non-architectural subsurface site is itself enormously complicated—and highly patterned. Surface indications proved to be remarkably poor predictors of the extent and nature of the subsurface site (fig. 24). Middens occur below areas of the lowest surface densities, while areas of high surface density often contained no subsurface site whatsoever!

We shall now attempt an explanation for this variability, with emphasis on what principles one might derive from the study of surface—subsurface site relationships. The surface site is the by-product of the operation of two independent realms of processes: cultural and environmental. The subsurface site is produced by the operation of cultural systems and to some extent by the simultaneous action of natural environmental forces. The surface site is produced by the continued operation of environmental processes on the site after the cessation of cultural ones.

In order to arrive at knowledge of the subsurface site one must couple knowledge of the surface site with an understanding of local environmental forces and a set of principles. The domain of principles which treats the relationship between the surface and subsurface sites has elsewhere been termed non-cultural formation process concepts (Schiffer, 1972a). Although every site has a unique depositional history, both during and after occupation, it may be possible to abduce from the Joint Site data, some principles which have general validity; principles which could be used for prediction on other sites.

Based upon our knowledge of the environmental conditions probably operative since the abandonment of the Joint Site in addition to knowledge gained through excavation, some hypotheses can be presented. The basic principle, known widely, but rarely seen in print, is that: *The identity of surface and subsurface sites increases where noncultural forces have not operated on a site after abandonment.* Because wind and water move materials downhill, *where an elevation gradient exists on a site (holding other variables constant) downhill surface material has a lower probability of indicating subsurface material.* Specifically obtainable from Joint Site data is the following principle: *Holding constant the durability of construction and preservability of raw materials, when wind and sand act upon a site containing standing walls, the taller the walls, the greater the distance from the wall(s) covered by a sand deposit.*

Examination of the area around the room blocks of the Joint Site discloses a generally low material density. Because of the height of the mounds we infer that the rooms probably have sufficient standing walls



Key

1 = 0 - 3.99/m²
 2 = 4.00 - 7.99/m²
 3 = 8.00 - 11.99/m²
 4 = 12.00 - 15.99/m²
 5 = 16.00 - 19.99/m²
 6 = 20.00 + /m²
 [] Schematic outline of room blocks



Scale

20 M

FIG. 24. Average density of lithic and ceramic counts for each provenience unit. Joint Site, NS 605.

to have collected a considerable sand deposit. Therefore, the surface around the room blocks is generally useless as an indicator of subsurface materials. Sand deposition on a midden would have produced the same surface as sand deposition on sterile soil. We conclude that there is a possibility (of unknown magnitude) that subsurface material may be found adjacent to the room blocks.

Examining the highly schematic contour lines placed on Figure 5 plus other surface indications leads to some additional statements about the subsurface site. The areas of medium material density to the west and northeast of the architectural areas lie on a slight downward slope which suggests that the source of the material lies closer to the pueblo—perhaps beneath the sand. The high density area to the northeast does not continue all the way southwest to the architectural areas. This discontinuity suggests that the source of this material may be isomorphic with the location of the surface remains. Testing indicates that some knowledge of the subsurface site would have been gained through the use of these principles. These examples in no sense constitute a test of the hypotheses since they were derived from the same data; they serve merely to illustrate the use of such principles.

The purpose of presenting these preliminary results is to suggest that the surface site is often deceptive and misleading; care must be taken to insure that variability in the subsurface site is adequately understood before intensive excavation is undertaken. One way to achieve such knowledge is by inference from surface remains through the use of the laws of noncultural formation processes. Another method is the use of trenches placed by earth moving equipment. Use of one or the other of these methods, perhaps in tandem, or in conjunction with others will allow more efficient placement of excavation units in non-architectural areas of a site.

In addition to revealing the structure of the subsurface site, especially the location of subsurface midden areas, the extra-architectural excavations uncovered a small reservoir (fig. 25). It is located to the southwest of the main room block and was discovered during the excavation of sample square S7W6. The north wall of this pit sectioned the reservoir approximately down the middle but not completely to the western edge. Examination of the profile revealed definite water-lain sediments, a portion of which appear to have been set down as varves. An extensive series of pollen samples was taken from this feature in the hopes that some information might be shed on seasons and length of use as well as the environmental events which occurred after site abandonment. Sherds



FIG. 25. Joint Site: Test square S7W6, north face. Arrow (50 cm. long) points north; meter stick in background.

dating from the projected occupation period of the site were found in all levels of this feature.

THE USE OF MULTIPLE RESEARCH DESIGNS AT ONE SITE

During the previous two summers the staff of Field Museum's Southwest Expedition decided that beyond the excavation of *a* site, an experiment was in order that we felt could, if successful, alter some precepts upon which archaeological research is based. It had been our general conclusion that most sites were excavated with only one research design. It is true of course that in many cases research designs were complex and multi-faceted. Oft-times we noted that experts from other scientific disciplines were included in the research efforts. Nonetheless, we felt that the problems and goals of most archaeological research were unidirectional for any given project. It was our belief that this was not always necessarily desirable. It seemed to us that given the time-labor-financial considerations that most archaeologists must take into account, the use of three fundamentally different research strategies was one way to minimize effort while maximizing information retrieval. Further, we

felt that by this method we would be able to say more about one site than singular research designs could.

It was with some trepidation that we undertook this project. We felt that either three major papers would be forthcoming from our investigations or that because of the problems inherent in our approach we would not recover all the necessary data for each of our research goals. This necessitated that we each compromise to some extent and we feel that this was an important factor in our excavation. It was at this point that one of our biggest problems arose. Because each investigator had ideas as to which data were important to him and each in some way or another was different from the other two, a stress situation was created. All the investigators directed excavations in different parts of the site during the first summer's work. In the beginning this did not always work to the advantage of the expedition. There were some discrepancies in the recovery and recording of various data classes depending upon the investigator in charge. These things we had not anticipated completely as the investigators were all directing an excavation for the first time. When our first preliminary analyses were completed we began to realize the necessary amount of planning that was needed to successfully complete a project such as ours.

This was the make or break moment for our experiment. Schiffer and Hanson were operating with a basically hypothetico-deductive method (see Hempel, 1966) while Gorman was experimenting with the applicabilities of an ethnoscientific eliciting technique and a basically inductive method of inference. Many long discussions as to the utility and efficiency of various excavation methodologies ensued. Out of these extended talks came one of what we feel to be the major advantages of an approach such as ours. Because of the differences in our theoretical orientations we were constantly questioning, arguing, re-evaluating and, ultimately, better understanding both our own and each other's particular needs and problems.

Because we wished to sample both the architectural and non-architectural areas, the second summer's field work was directed by only two of the investigators. The other was not "frozen out," but rather wished to pursue excavation at an earlier site. Because we had taken the time during the 1970 season to understand all the research strategies we were employing, the same classes of data were retrieved during the 1971 season. We would be less than candid if we did not admit that two directors on a site are easier to work with than three. In the 1971 season Hanson directed the excavations in the architectural areas and Schiffer in the non-architectural areas. We felt that this dual division would work

quite well for experiments such as ours. We still feel this way after completion of the excavation. In cases where the necessity for completing an excavation are not so immediate as in our own, or where a greater labor force is available, we feel that more than two "directors" *can* work on a dig and accomplish the goal of retrieving necessary data for multiple research designs.

On balance, we do not feel that our experiment was either an unqualified success or an unqualified failure. Two major factors hindered us. The first was the inexperience of the three principle investigators in planning and executing a project of this complexity. Secondly, at the beginning of the excavations we were not as familiar with the particular problems inherent in the research designs of the other two. *Some* valuable time and effort was lost until we sat down and worked them out. This was an oversight on our part and should not be taken as reflective of this experiment as a concept.

Despite these self-imposed hindrances we feel that the archaeologist can learn a great deal more from a site when more than one research design is being utilized. It is our contention that the days of "one-man archaeology" are over. Certainly we need professional colleagues in other disciplines to aid us. More than this, however, we feel that the future of scientific archaeology would be best served when researchers with diverse archaeological interests join in research efforts. This often necessitates some ego-shattering experiences but, ultimately, we feel, will result in better understanding the nature of the archaeological record, and hence, the human condition.

TABLE 8. Burial information chart.

Burial No.	Sex	Age	Burial Type
1	M	young adult 18 yrs.	primary, single, flexed, on left side
2	ind *	child approx. 8 yrs.	secondary, single, flexed, on left side
3	M	young adult approx. 20-25 yrs.	primary, single, flexed, on left side
4	ind	child approx. 5 yrs.	primary, single, rest unknown
5	ind	child approx. 8 yrs.	primary, single, flexed, on right side
6	F	young adult approx. 25 yrs.	primary, single, flexed, on right side
7	ind	adolescent approx. 12 yrs.	primary, single, flexed, on right side
8	ind	child 2-4 yrs.	secondary, single, rest unknown
9	M	adult 35-40 yrs.	primary, single, flexed, on back and right side
10	ind	adult approx. 30 yrs.	primary, single, flexed, on right side
11	ind	child 5 yrs.	primary, single, flexed, on left side
12a	ind	child 8 yrs.	secondary, rest unknown
12b	ind	infant or fetus	secondary, rest unknown
13	F	adult 35-40 yrs.	primary, single, semi-flexed (legs only), lying on back.
14	M	young adult	primary, single, semi-flexed lying face down
15	ind	child 5-7 yrs.	primary, single, flexed, on back

* ind = indeterminant

TABLE 8. (continued)

Burial No.	Completeness	Orientation of head	Pit context
1	complete but disarticulated by rodents	East	midden
2	complete but disarticulated by rodent activity	S. W.	midden
3	complete	N. E.	midden
4	unknown	unknown	midden
5	tarsals, mandible, and most of maxilla missing	East	midden
6	complete	East	midden
7	right arm missing	East	sterile caliche
8	very incomplete only recovered portions of clavicle, $\frac{1}{2}$ ischium, some of ribs, some vertebrae	N. E.	midden
9	complete	East	midden
10	complete	East	midden (deep)
11	several vertebrae, all foot and hand bones, patellas, most of clavicle, several ribs all missing	East	midden (deep)
12a	only skull, mandible and some phalanges recovered	East	midden
12b	only several ribs and vertebrae, part of skull cap recovered	unknown	midden
13	complete and completely articulated	East	(deep) midden (oven? kiln?)
14	complete, skull broken	East	midden
15	phalanges, most of mandible, arms except one humerus, maxillary all missing	East	midden

TABLE 8. (continued)

Burial No.	Number and kinds of grave goods	Remarks
1	1 brown patterned corrugated bowl, smudged interior 1 broken Show Low B/R bowl 1 whole Snowflake B/W bowl	shallow grave (due to run-off?), pit difficult to define
2	1 complete Snowflake B/W bowl	lies directly over burial 3, appears to be later than 3
3	1 whole Snowflake B/W bowl 1 whole Snowflake B/W pitcher 1 Snowflake B/W ladle 1 projectile point 1 broken metate	metate may be paint palette, point found in bowl, overlain by burial 2 and earlier than it
4	unknown	discovered and excavated by backhoe, skull and some long bones recovered.
5	none	none
6	1 brown patterned corrugated jar 1 Snowflake B/W bowl 2 McDonald's painted corrugated bowl	skeleton badly weathered, only burial discovered not in native caliche
7	1 gray indented corrugated jar 1 Show Low B/R bowl	skull and cervical vertebrae removed by backhoe (recovered), pit intruded by B8
8	none	appears to have been a bundle burial thrown on B7 or into its pit
9	2 St. John's polychrome bowls (one large, one small) 1 bone and turquoise bead bracelet	left leg and pelvis disarticulated by backhoe, body very robust - cranial deformation evident
10	1 plain redware bowl 1 gray indented corrugated jar 1 Snowflake B/W jar	hands and feet disarticulated, corrugated jar found within polychrome bowl
11	1 miniature jar (type?) 2 small projectile points	no rodent activity, so might be a bundle burial, jar contained one small human rib
12a	none	probably bundle burial
12b	none	buried with 12a

TABLE 8. (continued)

13	<ul style="list-style-type: none"> 2 Snowflake B/W bowl 1 McDonald's painted corrugated bowl (incomplete) 1 Show Low B/R bowl (incomplete) 3 bone awls (2 whole) 2 shell bracelets (one incised) 1 bead bracelet 10 flakes - various kinds of chert 	some ceramic offerings apparently broken by backhoe trench, richest burial, discovered bones excellently preserved
14	<ul style="list-style-type: none"> 1 St. John's Polychrome bowl 1 miniature Snowflake B/W jar, inside McDonald's 1 B/W jar Snowflake beads - scattered over body 	position of body indicates the individual may have come to an untimely end
15	<ul style="list-style-type: none"> 2 Snowflake B/W bowls 1 miniature Snowflake B/W jar 1 gray indented corrugated bowl 9 flakes - all possibly intrusive 1 shell bracelet 	bones not well preserved-cranial deformation, apparent rodent activity limited

VII

Stress Response in Cultural Systems: A Prehistoric Example from East-Central Arizona

by

JOHN A. HANSON

The scope of archeological investigations has increased significantly during the past decade. Up to the early 1960's most of those practicing archaeology were concerned with time-space systematics and a more explicit knowledge and delineation of material traits associated with the variously defined prehistoric and protohistoric "cultures." This was a world-wide phenomenon with many causes. In recent years a definite trend has been noted whereby something loosely labeled the "new archaeology" has appeared and currently flourishes. I have, for some time, wondered what the "new" was supposed to connote, as I never really learned the "old" archaeology. Many of the recent students at Field Museum's Summer Field Station likewise have exhibited this tendency. I think that it is safe to say that greater and greater numbers of "young" archaeologists (age in this case being a state of mind rather than some chronological determinant) are simply assuming that archaeology is and should be the anthropology of extinct sociocultural systems. We of the Field Museum Summer Staff feel that this is as it should be. It is our feeling that archaeology is anthropology or it is nothing. Realizing that there are numerous frameworks in which extant peoples can be and are studied, we do not wish to limit valid archaeological research because of theoretical orientation. This allows (as one example) for research focussing on ecological, social organizational, and cognitive variables to be carried out simultaneously on one expedition. This has been the case in Field Museum's Southwest Expedition. The investigators have been united in two ways: 1) in a concern with the understanding of how extinct cultural systems functioned and 2) with a framework which emphasizes rigorous application of the scientific method.

What follows is the outline of one such study (not yet completed) which has as its focus the responses systems make to prolonged environ-

mental stress. What is presented here is by no means definitive as few of the analyses have as yet been completed. Conclusions are preliminary and tentative, and much guesswork is involved. Nonetheless the presentation of the outlines of the study may be of some utility to others contemplating or currently pursuing similar questions and problems.

Plog (1969, p. 23) has outlined a typical research design that might be employed by archaeologists who are attempting to discover laws. There are six basic steps in this scheme: 1) acquisition of a hypothesis, 2) formulation of a research strategy, 3) acquisition of data, 4) analysis of data, 5) testing of hypothesis, 6) retesting, reformulation, evaluation of research.

While the specifics of individual archaeological investigation may necessitate some minor alterations in this scheme, as a general framework it is a useful means for organizing research. The present study is, at this writing, in steps 4 and 5. This paper will then detail what has been done in steps 1-5.

There will be no extended discussion of archaeological method and theory as regards archaeology in general and the study of prehistoric social organization and interaction, in particular. These subjects have both been covered extensively in the literature (cf. Binford, 1962, 1964, 1965, 1968; Martin, Rinaldo *et al.*, 1964; Flannery, 1967; Martin, Hill *et al.*, 1967; Hill, 1968, 1970a; Longacre, 1968, 1970; Dean, 1969a, 1970; Fritz and Plog, 1970; Schiffer, 1970; Tuggle, 1970; Martin, 1971, this volume; Winter, 1971; Zubrow, 1971a). Rather I will focus on the specifics of the present study. The method of organization will follow that of Plog's model.

The first step in any scientific research is to define a problem and acquire an hypothesis or set of hypotheses which can be used to explain the relationship between the variables under consideration. It must be remembered that hypotheses are not laws, but rather law-like statements which are capable of being tested. Winter (1971) has pointed out that any hypothesis implies a set of assumptions, ideas, and generalizations which makes the hypothesis and derived test implications a reasonable and worthwhile proposition.

The hypothesis under investigation¹ (adapted from Hill and Plog,

¹ By coincidence the general concepts involved in this set of hypotheses was derived by the author prior to his knowledge of the research design for the Ceramics Conference suggested by Hill and Plog. Because that research design and the authors' dovetailed to such an extent, it was decided to adopt their hypotheses for testing. He felt that testing these hypotheses on one site might be of utility to the general problems of stress response in the Southwest.

1970) suggests that during times of increasing or persistent environmental stress, a system will respond by increasing experimentation with technoeconomic and social organizational means of countering the stress—until the point at which a steady state is restored or the system collapses. Secondly, if the above is valid, it would suggest that as stress increases or persists there will be an increase in the scope of economic and social integration. This second suggests two corollaries:

1. As stress increases there will be increasing centralized decision-making—up to the point of steady state or collapse.
2. As stress increases, “social distance” will decrease proportionally (there will be more people involved and participating in given social institutions).

Before deriving some test implications for the above set of hypotheses some of the assumptions underlying the hypotheses should be discussed. This provides the reader with a framework in which to evaluate the reasonableness and utility of the hypotheses being tested.

Culture is here defined as man's extrasomatic means of adaptation (White, 1959). It is suggested that it is best viewed as a living system (Miller, 1965) composed of highly patterned and interrelated subsystems. Further, this patterning is reflected in the material remains of extinct sociocultural systems (i.e., the distribution of various material correlates will be non-random within a given archaeological site). Since systems attempt to reach and maintain a state of dynamic equilibrium the archaeological record can be of some import in ascertaining and explaining processes involved in changes in this dynamic equilibrium.

Culture has been defined elsewhere as an adaptive mechanism. As such, changes in the physical and/or social environments necessitate changes in one or another cultural subsystem in order for the system to maintain a steady state. Environmental stress is a change in the physical environment, and responses to these changes should be reflected in the patterning of the material remains of the extinct sociocultural system.

The major focus of this study is an attempt to measure the changes (if any) in intrasite social unit interaction as environmental stress increases and persists. The research design suggested by Hill and Plog is most concerned with regional patterns as opposed to localized ones. This is certainly an important question and a problem worth investigating extensively, but an intensive investigation of only one locality can be of profit for the feedback it would likely generate for those interested in wider regional consideration.

Listed below are some test implications which could be expected if the generated hypotheses are valid. It should be realized that the type of stress we are concerned with is environmental in nature. There is good palynological data to suggest that stress was intense in the Hay Hollow Valley especially during the period of A.D. 1100-1300, (cf. Hevly, 1964; Plog, 1969; Dickey, 1971; Zubrow, 1971a). During this period effective moisture was low and poorly distributed in the valley. A pattern of summer dominant rainfall was present, similar in many respects to the rainfall pattern today, i.e., intense but often short-lived thunderstorms with substantial run-off and arroyo cutting occurring. It is likely, therefore, that this brought about a depletion in energy available through the exploitation of cultigens (corn, squash, etc.), and necessitated alterations in the energy procurement and redistribution (economic) systems. It is assumed that the changes necessitated in one component or subsystem will bring about *some* changes in other sub-systems as well.

The first hypothesis above suggests that during times of increasing and/or persistent stress a system will respond by experimentation in both the techno-economic and social organizational sub-systems. It is assumed here that much of the experimentation would involve options already known to the people but not much utilized before the stress period ensued (cf. Wong, 1971). This is not to rule out the possibility of experimentation that was completely innovative. It would be generally less disruptive to the internal workings of a socio-cultural system were options employed of which some knowledge already existed. Some test implications for hypothesis 1 follow:

EXPECTED SOCIAL ORGANIZATIONAL RESPONSES

1. Increasing diversity of sizes of residence units (nuclear family, extended family, lineage (if present), etc.).
2. Increasing diversity of residence patterns (already investigated by Longacre and Hill in the Hay Hollow Valley).
3. Increasing variability between and among rooms (residence units) within a site.
4. Increased trade or exchange of techno-economic articles.
5. Decrease in length of site occupation.
6. Decrease in length of room block occupation within sites, much variability in length of occupation.
7. Increasing breakdown in age and sexual divisions of labor.

8. Increasing experimentation in ceramic design styles and types (including painted designs, shape, type).

EXPECTED TECHNO-ECONOMIC RESPONSES

1. Increasing diversity of subsistence crops being exploited as opposed to non-stress periods (especially non-domesticated food stuffs).
2. An increase in the number of kinds of exploited plants and animals which were marginal prior to the period of stress.
3. Increasing variety of tools utilized in obtaining and processing food. One would expect both an increase in the kinds of tools used as well as an increase in the variety of tools within functional classes.
4. Increased specialization in tool-making.
5. Increasing variety in sizes and shapes of rooms or areas within sites.
6. Increasing variety of techno-economic artifact clusters and/or factors representing more diverse tool kits or activity clusters.
7. Increase in storage capacity (one would expect more to be stored during times of stress). If various different kinds of foods were being stored we might expect an increasing diversity in the kinds of storage facilities being used—both in terms of spatial use and the kinds of vessels being used for storage.

The second hypothesis relates to increased integration and, if it is valid, the following test implication would be indicated:

EVIDENCE OF INCREASING SOCIAL INTEGRATION

1. Larger and fewer villages.
2. Increasingly large social units within villages (measured in numerous ways including design element distributions, number of rooms connected through walls, and the distribution of other kinds of features—firepits, mealing bins, height and width of doorways, etc.).
3. A generally increasing amount of stylistic sharing. This should be revealed in ceramic design styles between areas on a site as well as in such things as architectural styles within a site.
4. A decrease in the numbers of kivas or other ceremonial rooms, suggesting that more and more people were being integrated by fewer integrating situations.
5. Evidence of increased centralized decision making, a good indication of which might be noted in burial data, with the appearance of larger numbers of high status adult burials. We would also expect to find high status infant and child burials, suggesting

that one (or more) groups were present which conferred status from birth, rather than at initiation.

6. Evidence of increased centralization of communal activities. These activities might take place in spatially central locations, or in locations which might indicate that the activities were becoming increasingly shared. These activities would include storage, ceremony, processing, etc.

EVIDENCE OF INCREASING ECONOMIC INTEGRATION

1. Increasingly larger and more centralized storage facilities.
2. More, larger and increasingly efficient methods of water control.
3. Evidence of increasing task and manufacturing specialization.
4. An increased sharing of subsistence resources within the site. In this instance we would expect less variability in the proportionate amounts and kinds of food utilized between residence units.

This list of test implications by no means exhausts the possibilities which could be derived for testing the hypotheses. They are, however, sufficient to begin testing and I believed that they were of sufficient variability so that if changes were necessitated as a result of field-work these could easily be made as the situation warranted.

FORMULATION OF A RESEARCH STRATEGY

Most of the relevant material for this step is presented elsewhere in this volume, but the more salient facts should be recapitulated.

The excavation of a pueblo site with certain characteristics was desirable. It has to be large enough so that the definition of two or more residence (i.e., social) units was feasible. Second, a site dating to the stress period (A.D. 1100-1300) was needed. A third factor involved the accessibility of the site to roads, etc., as the summer rainy season can hinder the progress of an excavation if it is difficult to reach. Fourth, an undisturbed site (by pot hunters) was desired.

The site finally selected, the Joint Site, was one which seemed at first glance to fulfill all these qualifications, although initially I felt that it might be too small. This has not proven to be the case. For a detailed discussion of strategy, see Hanson and Schiffer, this volume.

ACQUISITION OF DATA

Because the problem of stress response necessitated a detailed knowledge of the range of plant and animal species being exploited, over 400

pollen samples were extracted for study. Ten large cartons of animal bone as well as 30 flotation samples were also collected. These were all submitted to Dr. Richard Hevly at Northern Arizona University for analysis.

Careful attention was paid to photographing and drawing all floors and walls as well as distinctive features. This procedure was also carried out with regards to burial data. A more detailed account of data acquisition techniques is presented in Hanson and Schiffer, this volume.

ANALYSIS OF DATA AND TESTING OF HYPOTHESES

For purposes of this paper I have grouped these two steps together. Because analysis is by no means complete, it is difficult to offer any but the most cursory conclusions regarding the testing of the hypotheses.

At present I am dealing with the ceramic material exclusively. All pottery from the architectural areas, burials, and test areas has been counted and recorded by type and frequency for each provenience level. Over 25,000 sherds and over 30 whole or restorable vessels were recovered from the Joint Site. At the time of this writing (April 1972) work is progressing on a design element distribution study. Both design elements and distribution of types will be submitted to factor and cluster analyses to attempt to place more precisely residential units within the site, as well as changes in residence that may have occurred over time.

The author's co-investigators (who are working on different problems) are analyzing the two other major classes of data, i.e., the chipped stone and ground stone.

We can now state with certainty that the Joint Site was occupied during the latter stages of the 12th century A.D. until at least the middle of the 13th. This corresponds to the severe stress period enumerated previously. It also means that the site is somewhat later than Carter Ranch but contemporaneous with Broken K. Therefore, other things being equal, the time range for the site's occupation suggests that it should be a good testing laboratory for the hypotheses.

Following are some of the tentative conclusions which seem to point to the positive testing of the hypotheses. It cannot be stressed too much that these results are preliminary and based more on observations by the author than on statistical tests.

Some evidence of experimentation is noted when the architecture and settlement patterns are studied. The rooms constructed toward the end of the occupation of the site are qualitatively different from the earlier

rooms. The early sets of rooms tend to occur in clusters of three, with a habitation room (with firepit), a preparation room (mealing bins and a mano-metate complex), and a storage room (featureless) making up the functional room sets. The activities seem to have been clearly delimited spatially. The latter end of the sequence suggests that rooms were being built that were intended to be multi-functional. Rooms with firepits *and* mealing bins are common. Three rooms were built that consist of two firepits plus processing tools. In one case (room 15), an early habitation room (with one central firepit) was apparently dismantled completely so that the larger room could be built. According to our calculations, this necessitated the complete removal of two and possibly three walls. The firepit was replaced by a pillar, apparently used to compensate for poorly constructed walls, which in turn weakened the roof structure (Charles Di Peso, pers. comm.). To compensate for the loss of this firepit, another was constructed south of the pilaster. These large rooms seem to have been built to accommodate more than one nuclear family. Storage rooms are not necessarily connected to the habitation-preparation room complex as previously. Another indication that some experimentation was taking place is suggested by the fact that of the three large two-firepit rooms, only one has any open doorways, and this one exception leads into a very small storage room.

The doorway-window complex suggests that many of the later rooms must have enclosed many activity sets as there is either *no* access at all through the walls or those doorway-windows previously existing were blocked up. This suggests that some experimentation was being carried out in the realm of social relations. While the large rooms suggest that integrative mechanisms of some kind were being employed, the patterns of access/egress indicate that residence (social) units were in some way isolating themselves from one another. It is hoped that further analysis will help clear up this seeming anomaly.

One further example of architectural experimentation is seen in the large kiva (room 34). Figure 21 shows a D-shaped subterranean room facing east. There are benches on the north, east, and south. The ventilator opened to the east. These features do not specifically set this special room off from others like it in Hay Hollow Valley, although there are some differences (see Hill, 1970b). The thing that makes this kiva unique from others in the area is the tunnel entrance in the northwest wall which opens in room 15. Kivas with tunnel entrances are frequently found in the Chaco and Mesa Verde areas, but this is the first instance in the Hay Hollow Valley or surrounding area. Further reference to this particular structure will be made in the discussion of integration.

Although analyses are at present incomplete, there would appear to be a certain amount of experimentation in ceramic design styles, particularly with reference to ceramic "type." Many would now question the utility of the type concept as an explanatory mechanism. Purely and simply, the definition of pottery types *per se* does not tell us very much beyond the fact that different types of pottery exist at X time in Y locality. Nonetheless, in any site or area a generally recognizable range of pottery types does occur. This is true of Hay Hollow Valley for the A.D. 1100-1300 time range. What is somewhat surprising at the Joint Site are the numerous unexpected combinations of previously known design styles (especially in terms of decoration and shape) which render existing categories meaningless. There is also evidence of experimentation in the production of the standard recognizable types, so that a ware such as St. John's Polychrome is often not recognizable by a reference to paint color. The basic red color of the St. John's ware exhibits itself in many variations, more so than would be expected on a site of this size. Some of this variation may be explained if future design analysis suggests that the pottery was being made away from the pueblo and was reaching it as a means (or an end) of some kind of regional interchange.

The work of Wiley (1971) has suggested that design elements on black on white pottery were being shared between sites. While it is too early to speculate as to the significance of this sharing, it is interesting to note that the coefficients of sharing are highest between the Joint Site in the marginal zone IV (Zubrow, 1971a) with other larger sites closer to the alluvial bottomlands (Zone VII). Zubrow has suggested that during this period resource availability was such as to necessitate population aggregation in the optimal resource zones which are the alluvial bottomlands. A suggestion exists here for some kind of social interaction, although the extent and significance of this interaction cannot yet be assessed.

Until artifactual and environmental analyses are completed, little more of substance can be said with regards to hypothesis 1.

Only some tentative suggestions regarding the tests of hypothesis 2 can be made at this time. Hypothesis 2 relates to integration. Survey data (see especially Plog, 1969; Zubrow, 1971b) indicate the presence of larger and fewer villages during this period. The 36 room Joint Site is one of the larger pueblo sites in the valley area but is different from most others of this period in its location (see above). Some population aggregation was apparently taking place at this time but the locus of this aggregation was in the bottomlands as opposed to the mesa on the east or the sandstone benches which flank the valley on the west. So then, while there *are* larger

and fewer villages, the Joint Site occupies a relatively special place in the distribution of Pueblo III settlements in the area.

Another test implication referred to an expected decrease in the numbers of kivas or special rooms suggesting that more people were being integrated by fewer integrating institutions. This would appear to have been the case of the Joint Site. The remains of four "kivas" were uncovered during Joint Site excavation. While only one was systematically excavated, it was apparent through test excavations that only one of the four was being utilized at the end of the site's occupation. The other three had clearly been abandoned. Two were filled with trash and the third had had rooms built over it. In fact, the D-shaped kiva previously described seems to have been the only special room of its kind in use at the time of abandonment. This reduction in number and the central location of the D-shaped kiva suggest that it was the major integrating institution operating in the later stages of occupation (no room kivas were discovered).

Analyses of the burial population have not yet been carried out but some tentative suggestions can be made. It would appear that at least some people had a higher status than some others (if sampling error is not a factor). Of the 16 burials, only five had no grave goods. All adults were buried with offerings but these varied in number and kind. It was evident that some children also were different from others. Of nine child burials, four were buried with grave goods. One child, approximately seven years old was buried with four ceramic vessels as well as one shell bracelet. More detailed analysis should indicate whether ranking was, in fact, a recognizable variable in the cultural system.

We cannot as yet speak directly to such things as the diversity, distribution, and sharing of subsistence resources, nor can we suggest as yet whether the test implications regarding activity loci, task specialization or distributions of various stylistic and functional artifactual variables will be borne out. There are certainly enticing suggestions that the hypotheses will be positively tested in some respects. It is also possible, and in some cases likely, that some test implications will not be borne out. It is only when analyses are complete that we will know whether or not the prehistoric inhabitants of the Joint Site were doing the kinds of things that the hypotheses suggest they should have been doing. If they were not, then we will need to reformulate and/or modify the hypotheses and perhaps the assumption base from which they were generated.

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VIII

Behavioral Chain Analysis: Activities, Organization, and the Use of Space

by

MICHAEL B. SCHIFFER

INTRODUCTION

The view once widely held that archaeological remains must remain forever silent on the subject of social organization is seldom placed in print today. Since the early 1960's, numerous studies have demonstrated that it is possible to design methods that can successfully retrieve information on topics such as a breakdown in matrilineal residence (Deetz, 1965), the identification of uxorilineal residence units (Longacre, 1964, 1970; Hill, 1966, 1970a, b), differential acculturation of sex roles (Deetz, 1963), and the functional differentiation of puebloan rooms (Hill, 1968, 1970a; Dean, 1969a), among many others.

Aiming to provide a general method for discovering how puebloan task units structured their activities in space, Hill (1970b) has generalized the method he used at Broken K pueblo to demonstrate the similarity in the past patterning of activity space (and presumably local groups) with patterns observed among the ethnographically-known western pueblo groups. This method leads to (or from) hypotheses concerning the organization of households and other local groups in terms of the recurrent sets of activities that were conducted within similarly patterned sets of bounded spaces (pueblo rooms). A major portion of this method is aimed at demonstrating that certain formal differences in pueblo rooms were related to differences in function (i.e., the activities that were conducted there). On the basis of the artifact and ecofact content of similar room types among western pueblos, a set of test implications is deduced for the archaeological case. The fit between expectations and actual archaeological evidence is compared, and the functional hypotheses are accepted, modified or rejected.

The purpose of this paper is to point out several weaknesses in Hill's method that might possibly make it unsuitable for more general use in

its present form. Of primary concern are the two important problems posed by cultural formation processes (Schiffer, 1972), on the one hand, and variations in activity space partitioning, on the other. These problems are discussed in some detail and Hill's method is modified to take them into account. The modified approach makes use of *behavioral chain analysis*—an explicit procedure for hypothesizing past activities and deducing their archaeological test implications.

CULTURAL FORMATION PROCESSES

A principal assumption of Hill's method is the following:

... where different kinds of activities are carried out within a community, one would expect to find different kinds of artifacts; and the presence of different artifacts in particular rooms or areas within an archaeological site should be usable as evidence in inferring the activities of these rooms and areas—assuming that one can identify the uses of the artifacts involved (Hill, 1970b, p. 19).

If one proceeds on the basis of this assumption, at least one additional assumption has been implicitly accepted: artifacts are discarded at their locations of use within a site. While this may be true for some kinds of sites (Schiffer, 1972), in many sedentary villages and larger sites highly developed refuse transport and discard systems must have been developed. This set of variables relates to the important but usually untreated problem of cultural formation processes—how does the *systemic context* of artifacts and features (their participation in a past behavioral system) relate to their *archaeological context* (Schiffer, 1972)? In examining this problem I have found it useful to distinguish between three fundamentally different kinds of refuse (Schiffer, 1972). *Primary refuse* is discarded at its use location, while *secondary refuse* is transported away from the location of use and discarded elsewhere. *De facto refuse* consists of those items which are not discarded during the normal operation of a cultural system but abandoned as the occupants leave the site. Although most of the material at Broken K is secondary refuse, enough primary and *de facto* refuse was present to permit the successful application of Hill's method. But this is not so at all sites.

The implicit merging of archaeological context and systemic context can be seen at several points in Hill's (1970b) paper. For example, in discussing how test implications may be derived from ethnographic data he suggests that:

An examination of the activities performed in the modern room types would then yield a series of test implications for each (in terms of artifacts and their relative

frequencies and spatial distributions), and the investigator would turn to the archaeological evidence to determine the degree to which his expectations are met . . . (Hill 1970b, p. 30).

In the cases where he presents test implications for functionally different rooms, they are simply statements about what is found in rooms of the same type occupied by the ethnographically-known Hopi and Zuni. Unfortunately, the pueblo (or other) structures we excavate no longer are part of an ongoing behavioral system; and our methods must take account of the way such systems produced archaeological remains.

ACTIVITY SPACE PARTITIONING

The second major problem in generalizing Hill's method concerns the wide cross-cultural variety in the way that social units partition their activity spaces. In other words, there may be more or less compartmentalization in the partitioning of activity space by the systems that generated other sites. What we now require is a means by which we can discover how any society, pueblo or non-pueblo, spatially bounded its recurrent activity sets. In many cases the use of ethnographic analogies will not permit restriction of the range of possibilities. Ideally, the modified method must be potentially free from dependence on *specific* sets of ethnographic data.

Hill briefly addresses the problem of discovering patterns of room use different from the Broken K paradigm. He offers the following two approaches for describing intra-community task organization:

The first is to begin by describing the variability in room types, plazas, and other areas within the site and then ask the question, "What kinds of activities were being performed in these rooms and areas?" The other approach is to begin with a specific list of *activities* that are of interest to the archaeologist and ask the question, "Where were these activities being performed?" (Hill, 1970b, pp. 28-29, emphasis in original).

The latter approach, discussed briefly in the context of addressing the problem of "missing activities," has the potential for discovering previously unsuspected patterns of space utilization.

This situation [missing activities] may be avoided by focusing less attention on the test implications of rooms and areas and more attention on the test implications of individual *activities*. In this approach, the investigator would first consider the kinds or clusters of cultural remains that are expected to have been associated with particular activities and then study the spatial distributions of these clusters within the site (Hill, 1970b, p. 32, emphasis in original).

This approach, when divorced of its implicit merging of archaeological and systemic context, appears promising because it is the distributions of the activities themselves that are being compared. A modification of this approach, based on explicit consideration of cultural formation processes will now be elaborated.

In constructing the general method, several assumptions are made. The first is that we possess data from an already excavated site. It is further assumed that a rigorous program of sampling was applied and all data relevant to the problem at hand were recorded. And finally it is assumed that the site has a known regional context, and information from other excavated sites can be brought into the analysis.

BEHAVIORAL CHAINS

The first task at hand is the determination of what activities took place at the site. Only after these have been identified can the question be raised concerning their locations in space. After excavation of the site, analysis begins through the application of hypotheses gleaned from general anthropological knowledge and regional information. One way of ordering and extending these activity hypotheses is by narrowing down from broad categories to more specific activities in the "life" of all elements of the past cultural system. Using broad categories of basic processes intersected by major classes of cultural elements (see Schiffer, 1972, for the basic processes) one can derive a hierarchy of activity sets (fig. 26). Once one has deduced a specific process in the systemic context of a single element, the problem is then visualized in terms of *behavioral chains* and their *chain segments*. Behavioral chains lead directly to archaeological test implications for determining whether or not a specific activity was conducted at a site.

A behavioral chain is the sequence of all activities in which an element participates during its "life" within a cultural system. A chain segment is then simply a specified portion of a given chain. For the sake of convenience, the example used throughout this paper derives directly from ethnographic data. A partial behavioral chain of maize for the Hopi (circa A.D. 1900) has been reconstructed from the works of Bartlett (1933, 1936), Stephen (1936), Beaglehole (1937), Whiting (1939), and Turner and Lofgren (1966). Unfortunately, none of these ethnographic accounts is adequate for the purpose at hand. Even though a composite chain was drawn from the data of the several villages at several points in time, many entries had to be guessed at. Table 9 presents the completed chain in which all plausible entries made by this author are indicated by a

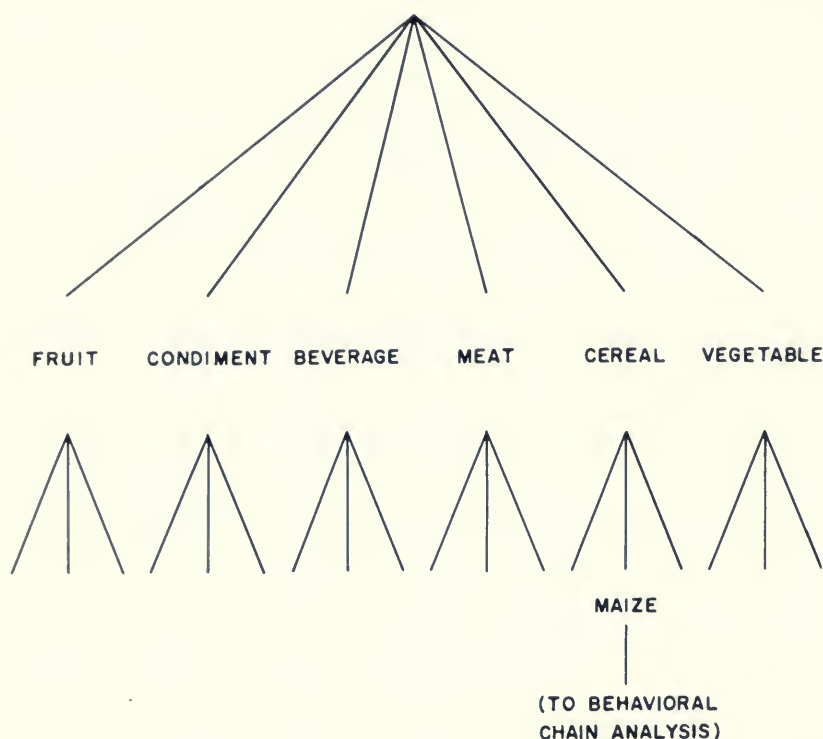


FIG. 26. A hierarchical taxonomy of food preparation activities of the Hopi, ca. 1900.

lack of explicit citation to other authors. The ultimate accuracy of this particular case is irrelevant for illustrating the basic principles of construction and use of behavioral chains.

Behavioral chains are not new in archaeology. I have simply made explicit and given a name to one form of reasoning employed to arrive at the activities that were performed at a site and their test implications. In this presentation, I have been heavily influenced by Harris (1964). My emphasis on the life-history of system elements differs somewhat from the actor-activity orientation of Harris. The reason for this shift in perspective relates to the material nature of the archaeological record and some of the predictive properties of behavioral chains—properties that permit the investigator to circumvent the apparent limitations of the archaeological record.

Although the actual behavioral chain is represented by the sequentially related activities in the systemic context of any cultural element,

TABLE 9. Partial behavioral chain of maize for the Hopi, circa A.D. 1900

ACTIVITY	ENERGY-SOURCES	CONJOINED ELEMENTS	TIME AND FREQUENCY	LOCATION	OUTPUTS	INTERSECTIONS
	SOCIAL UNITS NON- HUMAN					ADDITIONS DELETIONS
HARVEST	ABLE VILLAGERS OF BOTH SEXES 3, 4	BASKETS 4	SEVERAL DAYS IN SEPTEMBER 4	FIELDS OF H. H. 3, 4	STALKS, WASTED OR UNHARVESTED MAIZE	
TRANSPORT	ABLE VILLAGERS OF BOTH SEXES 3, 4	BASKETS BLANKETS 3, 4	ONCE IN SEPTEMBER	FROM FIELDS TO ROOF OF H. H. AREA	POLLEN	
HUSK	WOMEN OF H. H. AND OTHER FEMALES 3, 4	WOODEN OR BONE PEG 6	ONE OR SEVERAL DAYS IN SEPTEMBER	ON ROOF OF H. H. AREA 3, 4	POLLEN	HUSKS
DRY	4 SUNLIGHT	ROOF OF H. H. AREA 4	SEVERAL DAYS IN SEPTEMBER	ON ROOF OF H. H. AREA 3, 4	POLLEN	
TRANSPORT	WOMEN OF H. H.	BASKETS	ONCE IN SEPTEMBER	FROM H. H. AREA TO STOREROOM	OCCASIONAL KERNELS, POLLEN	
STORAGE		STOREROOM 3, 4, 6	1 TO 100 WEEKS · 6	STOREROOM 3, 4, 6	OCCASIONAL KERNELS, POLLEN	
TRANSPORT	WOMEN OF H. H.	BASKETS	SEVERAL MORNINGS WEEKLY	FROM STOREROOM TO HABITATION ROOM		
REMOVE KERNELS	WOMEN OF H. H. 3	SHORT STICK, YUCCA BASKET 3	SEVERAL MORNINGS WEEKLY	HABITATION ROOM	OCCASIONAL KERNELS, POLLEN	COBS
COARSE GRIND	WOMEN OF H. H. 1	MEALING BIN, STICK, COARSE MANO AND METATE, YUCCA BASKET 1, 2	SEVERAL MORNINGS WEEKLY	HABITATION ROOM 1	WASTED KERNELS AND MEAL POLLEN	
REMOVE CHAFF	WOMEN OF H. H. WIND	YUCCA BASKET 3	SEVERAL MORNINGS WEEKLY	OUTSIDE OF STRUCTURE	CHAFF	
MEDIUM GRIND	WOMEN OF H. H. 1	MEALING BIN, STICK, MEDIUM MANO AND METATE, BOWL 1, 2	SEVERAL MORNINGS WEEKLY	HABITATION ROOM 1	WASTED MEAL	
FINE GRIND	WOMEN OF H. H. 1	MEALING BIN, FINE MANO AND METATE STICK, BOWL 1, 2	SEVERAL MORNINGS WEEKLY	HABITATION ROOM 1	WASTED MEAL	
TRANSPORT	WOMEN OF H. H.	BOWLS 2	SEVERAL MORNINGS WEEKLY	HABITATION ROOM TO STOREROOM	WASTED MEAL	
STORAGE		BOWLS 2	SEVERAL DAYS TO A WEEK	STOREROOM	WASTED MEAL	
TRANSPORT	WOMEN OF H. H.	BOWLS 2	TWICE DAILY	STOREROOM TO HABITATION ROOM	WASTED MEAL	
MAKE DUMPLINGS	WOMEN OF H. H.	COOKING JAR, BOWL	TWICE DAILY	HABITATION ROOM	WASTED MEAL	WATER, OTHER INGREDIENTS
COOK	FIRE	JUNIPER TWIGS COOKING JAR FIRE PIT · 6	TWICE DAILY 3	HABITATION ROOM	SPILLAGE - WASTE	
SERVE	WOMEN OF H. H.	SERVING BOWLS COOKING JARS LADLES · 5	TWICE DAILY 3	HABITATION ROOM	SPILLAGE - WASTE	
EAT	ENTIRE H. H.	BOWLS 5	TWICE DAILY 3	HABITATION ROOM	WASTE	
DIGEST TRANSPORT	ENTIRE H. H.		ALMOST CONTINUOUSLY	LOCATIONS OF H. H. MEMBERS		OTHER FOODS
DEFECATE DISCARD	ENTIRE H. H.	A BROAD LEAF	ONCE DAILY	AWAY FROM OCCUPIED ROOMS	A BROAD LEAF, RESIDUES	

KEY

- 1 BARTLETT (1933) 4. STEPHEN (1936)
 2 BARTLETT (1936) 5. TURNER AND LOFGREN (1966)
 3 BEAGLEHOLE (1937) 6. WHITING (1939)
 H. H. HOUSEHOLD

behavioral chain analysis consists in part of hypothesizing and using the *components* of each individual activity. The smallest segment of a behavioral chain is a single *activity*. An activity is defined as the patterned interaction between at least one energy source (human or non-human) and at least one other cultural element (Schiffer, 1972). Each activity is described for the purpose of behavioral chain analysis by listing the following *components*:

1. A behavioral description of the activity.
2. The nature of the constituent human and/or non-human energy source(s).
3. The element(s) conjoined or associated with the one under consideration.
4. The time(s) and frequency of activity performance.
5. The location of activity performance.
6. The pathways created to the archaeological record by the outputs of activity performance.
7. The points at which other chains of elements integrate with or diverge from the element under consideration.

These essential components of any activity will now be given more explicit definition. One is again referred to the example of a behavioral chain segment of maize among the Hopi (table 9).

Activity definition

The nature of the activity is one of the most important components. Activities should be described in terms of the dynamic relationships among the various interacting elements. For example, in using the term "grinding" one is attempting to be precise in designating a set of behaviors. Grinding implies that the object of the activity, such as maize, is being worn down by the application of a tool. Because the attributes of tools make them more or less suitable for being used in a certain way, the precise specification of behavior can lead to a listing of the attributes a conjoined element must have possessed (or acquired through use). These inferences are made possible by application of general principles (correlates) that state relationships between attributes of objects, behavior, and results of behavior in terms of the attributes of the objects (Schiffer, 1973). The construction and experimental testing of correlates depends not only upon looking at activities differently but also developing a specialized language for describing behavior. Such terms as "grinding," "pounding," etc., may turn out to be hopelessly crude for use in the kinds of predictive principles archaeology must and will eventually possess.

Energy sources

The number and nature of human energy sources is a designation of the social unit of activity performance (cf. Freeman, 1968). This unit consists of any and all humans associated with the element during a specific activity. The concept of social unit of activity performance applies minimally on two levels: the individual, and the societal level which is recurrent. For example, one might point out that among the Hopi a post-pubescent woman does the coarse grinding of maize. At another level of analysis, one might want to specify that the recurrent social unit of maize-coarse-grinding is all post-pubescent women of a society. One can shift the level of analysis to suit specific needs. It should also be possible to consider and describe patterned internal variability in a society for a social unit of activity performance. Non-human energy sources include the sun, wind, fire, machines, animals, etc.

Conjoined elements

Conjoined elements are those associated with the one under consideration during an activity excluding the energy source. They should be conceived of in terms of the attributes critical to their interaction (cf. Dunnell, 1971). This implies that not all attributes of form are relevant for the description of an element or its identification. For example, "maize coarse-grinding" requires at least two elements with hard surfaces capable of breaking the endocarp of maize kernels, at least one of which is capable of sustained manipulation by the human energy source. Clearly, not all objects at a site will possess the requisite attributes; of those that do, one can select the correct elements on the basis of other attributes such as wear, etc. This hardly begins to exhaust other classes of data for testing "functional hypotheses," such as association, spatial location, or quantity and relative frequency.

Times and frequency

The times and frequency of activity performance are relatively easy to define (but often very difficult to determine). Reference is made here to the class of usual performance times and frequencies with the stipulation that variability can be encompassed in specific applications. As an example, among the Hopi the activity of metate stone procurement took place once yearly, in the winter (Bartlett, 1933).

Location

The location of activity performance ordinarily refers to a location or class of like locations within an area; they can be specified relative to each other or with respect to stationary features.

Outputs or pathways

At every point in the behavioral chain that is labeled "output" a path exists through which materials may become a part of the archaeological record. In the case of outputs such as waste during storage, including pollen grains and some seeds, the material may undergo no further cultural transport or discard. Other pathways are more complex. For example, waste products from cooking or mixing activities constitute an obvious inconvenient and unsanitary residue that would most likely be cleaned up, transported, and discarded as secondary refuse. In societies with highly developed refuse disposal systems, most elements make their way into the archaeological record at locations other than those of their use, and it is necessary to specify in the output component of the behavioral chain exactly how and where these discard activities take place (this has not been done for the Hopi maize example).

In addition to the pathways by which waste products begin their way into the archaeological record during activity performance, there exists an additional and extremely important source of outputs. Elements conjoined with the one under consideration in an activity may terminate their use-life during an episode of activity performance. If each instance of activity performance is defined as one use for all constituent elements (except consumables Schiffer, 1972), then the quantity of any element terminating its use-life during an instance of activity performance may be expressed as follows:

$$C = 1/b$$

Where,

C = the number of elements exhausted during one instance of activity performance. This variable is termed the *output fraction*.

b = the total number of uses of which an element is capable during its use-life. In cases where reference is made to a class of like elements, b designates the average number of uses per use-life.

As a result of this relationship, one would expect an instance of activity performance to create the following pathways to the archaeological record (when there is no re-use):

$$Y = C_1 + C_2 \dots + C_n$$

Where,

Y = the total number of elements ($1 \dots n$) exhausted during one instance of activity performance.

$C_1 \dots C_n$ = the respective output fractions of all elements ($1 \dots n$) of an activity.

The principles for describing and explaining the various pathways to the archaeological record are in their initial stages. The above equations are part of a network of laws purporting to explain some cultural formation processes of the archaeological record (Schiffer, 1973).

Chain intersections

In constructing the behavioral chain of the element under consideration, it may be necessary for some problems to specify when another element has become attached, or when a diverging chain segment is created. For example, spices and other ingredients become a part of the maize behavioral chain during "dumpling preparation" activities. In the case of divergence, one can cite the separation of kernels and cobs; the latter forms a new divergent chain segment (fig. 26).

BEHAVIORAL CHAIN ANALYSIS

The above discussion, it must be emphasized, presents a framework for describing the interrelations between behavioral and spatial-material aspects of activity performance with reference to the life-history of cultural elements. This orientation demands that the description of activities performed at an archaeological site be expressed in terms of highly specific hypotheses on an empirical, behavioral level. To deduce the test implications of so broad an activity category as "food preparation" (fig. 26) is a trying if not impossible task; evidence for such a demonstration is always ambiguous. But, with more exact definition of activities one is led to consider conjoined elements, spatial location, and outputs, thereby facilitating the task of specifying relevant test data. By postulating past activities and describing them in terms of behavioral chain components, one can follow the cultural pathways to the archaeological record and make activity documentation more secure.

In addition to the cultural pathways of archaeological record formation, there exists a set of non-cultural formation processes which may eliminate elements (organic decay, etc.), modify them (natural deposition, weathering), or redeposit them. One can extend the concept of behavioral chain to include these processes. The resulting chains contain pathways that lead directly from the performance of activities in the past to the actual proveniences of material in the archaeological record (or the point in time when they disappear). I have not failed to notice the implications of these formulations for constructing simulation models (Schiffer, 1973).

An important property of behavioral chains lies in their ability to facilitate the prediction of activities that, although not directly indicated,

must have occurred at a site. To justify this claim, three general principles are offered below. Quite clearly, these principles have many exceptions and are not universally applicable, but it is hoped that their judicious use will lead to many specific hypotheses about the activities conducted at a site, and the kinds of data that might confirm their presence.

The first principle is:

When two non-sequential activities in the behavioral chain of an element occur at a site, then the activities that took place between them on the chain also occurred at that site.

For an example, let us return to the behavioral chain of maize among the Hopi. If it is known that storage (between drying and kernel removal) took place at a site, and one finds coprolites with direct evidence for the consumption and discard of maize, then one can deduce by the above principle that the intervening activities of grinding, mixing, and cooking also took place at that site. The past occurrence of these hypothesized activities can be determined on basis of independent archaeological evidence implicated by output components.

Use of the above principle in this manner presupposes the availability of sufficient data to create generalized behavioral chains for different kinds of elements. At present, we can operate only at very general levels, and only for a few kinds of elements. Experimental studies, ethnoarchaeology, and a careful search of extant ethnographies should provide data to build additional chains which can be used as models for archaeological testing. James T. Rock (pers. comm.) is constructing behavioral chains for such elements as utilitarian pottery and foodstuffs in western pueblo societies. Many of these chains may be useful beyond puebloan society since they rest ultimately on general principles of human behavior.

Another much weaker principle is offered here whose careful use may allow further generation of activity hypotheses.

If one activity in the life history of an element occurred at a site, then the activities which followed it on the behavioral chain (to and including discard) also occurred at that site.

Again, great caution is urged in its application, but as a device used simply for obtaining hypotheses, it should serve well.

When dealing with stationary elements, an additional principle is presented that will facilitate numerous behavioral deductions:

For stationary cultural elements, all activities of the behavioral chain segment from the beginning of manufacture through discard

occurred at that site, and probably at that location (adapted from Schiffer, 1972, p. 161).

One would expect this principle to hold true because the behavioral chains of stationary elements, such as a pit or house, have a constant locational component (at least for activities of manufacture through discard). Let us turn, for example, to the ubiquitous subterranean storage pit. From the presence of such a facility on a site one can infer the past occurrence of specific manufacture and use activities. Because pits of this type must have been excavated by the site occupants with, most likely, a pointed object (and this can often be tested by direct examination of the pit's wall), one can thereby deduce that at least one other element (e.g., an antler, wood, or stone digging tool) was conjoined with the pit during its initial excavation. The elements conjoined can be determined by the use of behavioral-material correlates. These principles indicate the attributes that an element possesses in order to be used in a specified behavior.

To this point in the presentation it has been taken for granted that one can identify with relative ease the constituents of an activity that has been inferred through the use of behavioral chains. Let us return to the activity of maize cooking; the investigator desires to know what were the conjoined elements of maize in the cooking activity. By applying the relevant behavioral-material correlates to deduce properties and attributes of some of the conjoined elements, the nature of the conjoined elements can be determined with a high degree of probability by using site- and region-specific information.

Cooking, by boiling, is an activity in which chemical and physical changes are produced in consumable elements through the indirect action of a heat source. The occurrence of such an activity requires minimally a source of stored energy, a location for its transformation into heat, and a facility for containing the element to be cooked, one that is resistant to heat. Given knowledge about the main artifact classes of the site, and of other similar sites, one can select the most likely element from among the feature classes as the one used to produce heat for maize cooking. In the case of the Joint Site (Hanson and Schiffer, this volume), this might be a firebox or firepit.

A second implication of this type of cooking is that the heat source is not in direct contact with the consumable. We would expect the presence on the site of a facility capable of withstanding as well as transferring heat. Knowledge of the site contents would lead one to examine the pottery, from which certain types (painted, but unglazed wares) could be immediately eliminated from consideration on the basis of their inability

to withstand repeated contact with heat. Employing some of Linton's (1944) behavioral-material correlates, one can identify the vessel shapes most suitable for this type of cooking from the remaining pottery classes. Ordinarily, one would not boil food in a bottle, dish, or uncovered bowl. At this point, the probable cooking vessels might be limited to several different sizes of a given shape (wide-mouth jars), within a particular pottery class (corrugated). The remaining size variation might be attributable to differences in either cooking activities or social unit size (Turner and Lofgren, 1966). Additional specific testing on independent data (such as soot and other chemical residues) might support these inferences. Further testing is facilitated by the use of behavioral chains, especially the output components, and usually, behavioral-material correlates, behavioral-material-spatial correlates, and *c-transforms* (principles that describe cultural formation processes).

The identification of conjoined elements is not as difficult as might initially be expected, but does require considerable knowledge of extant correlates and site- and region-specific information.

Beginning with only a few cultural elements, an investigator can, by following the ramifications and intersections of behavioral chains, touch one or more times on every activity conducted at a site. In practice, such comprehensive reconstructions are seldom attempted for the obvious reasons that they require very broad interests and ample analytical resources. The use of behavioral chain analysis for a large-scale activity reconstruction would facilitate a fine-grained analysis of space utilization at a site.

ACTIVITY LOCATIONS

Given a possible range of discard activities and locations at a site, the problem remains to determine from the distributions and associations of elements, where they were used in their respective activities.

Let us begin with those activity areas subjected to complete removal and transport of refuse. Under such conditions, only materials not susceptible to the available technology of refuse removal and transport would be expected to remain at locations of activity performance. For example, pollen should be found where maize is stored or husked. In addition, if no recycling or scavenging activities (Ascher, 1968; Schiffer, 1972) occurred, fixed features (such as pits, hearths, and mealing bins) should also be found at their use locations. The investigator must determine from the available data and behavioral chain activity hypotheses

the most likely activities that were conducted in these locations. In other parts of the site (or at other times during site occupation) refuse disposal systems could have operated imperfectly, or have been poorly developed, producing quantities of primary refuse. Under these conditions, and when *de facto* refuse is also produced, one can deduce with greater certainty the occurrence of activities at a location. One begins with the assumption that all materials within the bounded (presumably habitation) spaces are found at their locations of use (except for those abandoned spaces—used as dumps). This means that the present location of such elements can only be accounted for in terms of their past participation in certain activities there. The presence of some material elements may be sufficient to suggest the occurrence of one and only one activity, while others may of course be involved in several activities and implicate (potentially) them all.

What one must do first is examine activity spaces for the most specific indices of activities and record their presence among the activity spaces. Next one turns to those elements or element fragments that indicate more than one activity. Using behavioral chains and relevant correlates, one can identify the other activities likely to have been spatially associated with the one under consideration. The identification of spatially-associated activities provides one way of discriminating between alternative possibilities.

The most difficult set of data to work with in attempting to identify activity locations is secondary refuse. In an earlier paper (Schiffer, 1972) I have suggested some of the conditions under which one might expect secondary refuse associations to be based on associations of elements within the use process. These hypotheses are still untested, but may be useful here if only as assumptions awaiting further testing. One must first determine the relationships between activity locations and secondary refuse deposits. By consulting the conjoined elements and output components of the behavioral chain and applying some of the above-mentioned hypotheses, it is possible to deduce probable secondary refuse associations. In any case, the exact nature of the refuse storage, removal, transport, and discard activities must be stipulated in order to predict associations and other patterns within secondary refuse. The verification of such predictions is likely to remain at a very crude level until excavation techniques have been adjusted to the range of questions raised by a study of this sort. It should be possible to excavate secondary refuse so as to recover discrete discard episodes (materials that were discarded at the same time and by the same social unit).

To optimize the information potential of each available kind of refuse one should design a multi-phase testing procedure. First, employing be-

havioral chain analysis, one arrives at a listing of the probable activities that were conducted at a site. The next question is to locate these activities in space. Using all material suspected to be primary or *de facto* refuse, an attempt is then made to identify as many of these activities as possible at these locations. Next, one deduces further implications for the secondary refuse. By this multi-phase testing procedure it should be possible to arrive at highly credible statements about activity locations, as well as explain the occurrence of many classes of data in the archaeological record (and their spatial and associational patterns).

ORGANIZATION AND ACTIVITY SPACE

After all channels of reconstruction have been exhausted and the likely activity constituents of any and all units of bounded space (or any units for that matter) at a site have emerged, the question can be raised as to the possible existence of regularities in the data that might correspond in some way to functional differentiation in the use of these spaces. To answer this question, one must apply an appropriate computer-aided analysis to determine which rooms or spaces are similar in terms of the activities performed there. Although it would be preferable to have a computer program designed specifically for the analysis of archaeological data, either factor or cluster analysis should be adequate for the task at hand. More refined studies must await the development of archaeological statistics.

If activity spaces contained overlapping sets of activities, a factor analysis of these activities with respect to rooms should bring out sets of related activities. The factor scores on each room indicate its approximate activity composition.

A cluster analysis of rooms with activity variables will yield a typology of rooms, indicating similarity of activity composition among rooms within a cluster. In most cases, it is advisable to attempt both factor and cluster analyses on the data.

Because of the complex patterns of cultural formation processes that operated to produce the remains of sedentary village systems, a statistical analysis of artifacts and features alone, even if found in floor proveniences, is likely to give erroneous or incomplete results. Activities were differentially partitioned in space; not artifacts. It should be stressed that *activities*, not artifacts, must be clustered. This point cannot be over-emphasized.

Once one possesses a room typology based on an understanding of

how activities were structured within bounded spaces, it can be hypothesized that such units are in some sense related to various resident social units.

The notion of recurrence in activity sets is the basic clue to discovering the past patterns of social organization (for most cultural systems). For, the detection of recurrent activity sets, performed in analogously partitioned spaces, suggests similarity in the social units responsible for these activity sets. The most abundant room types (or activity factors) are likely to be associated with domestic or commensal units of some sort.

One potentially valuable result of the statistical analysis may be the occurrence of exceptions or anomalous cases. It is these exceptions that may provide clues to meaningful variability in the activity structure that can serve as a basis for generating or testing further hypotheses about past organization. It is also necessary to consider explanations for variability in the use of space caused by developmental cycles in domestic groups (Wilcox 1971; Rock, 1972), and "devolutionary cycles" (David, 1971). These organizational hypotheses can be tested on the myriad sets of unused data that remain from the activity analysis. These might include, for example, the location of different kinds of rooms with respect to each other, patterns of construction (see Wilcox, this volume), patterns of doorways and communication (Rohn, 1965, 1971), and design attributes of element classes (Hill, 1970a; Longacre, 1970).

SUMMARY OF METHOD

The method outlined above can be simplified into a set of steps for deriving the past spatial partitioning of activity sets, and the generation of organizational hypotheses to explain them.

1. General anthropological knowledge and site- and region-specific information yield broad classes of activities.
2. Behavioral chains, correlates, and c-transforms and all data classes yield a list of specific activities conducted at the site.
3. The list of site activities, behavioral chains, correlates, c-transforms, and primary and *de facto* refuse lead to statements about the activities conducted per unit of activity space.
4. The list of activities, behavioral chains, secondary refuse data, correlates, and c-transforms yield additional activities for each unit of activity space.

5. Statistical analysis of activities and activity spaces produces room types and major sets of recurrent activities.

6. Room types and activity sets provide basic data for hypothesizing aspects of social organization.

7. Organizational hypotheses, correlates, c-transforms, and other sets of data, especially stylistic, yield tests of hypotheses.

8. Examination of residual room types or unexplained activities, and negative tests in 7, are recycled to step 6 and repeated until the organizational hypotheses provide a best fit to the archaeological data.

CONCLUSION

Behavioral chain analysis, a technique of determining past activities and their performance locations at a site, has been applied to generalize the approach to behavioral and organizational reconstruction that Hill found so useful at Broken K Pueblo. Behavioral chain analysis provides a means to cope with cultural variations in the way that activity space is structured and used, and more importantly, indicates how an investigator can take into account different and complex cultural formation processes. These modifications of Hill's method do not provide a polished program for organizational reconstruction at any site, but are intended to serve as a trial research design subject to experimentation, criticism, and revision. Behavioral chain analysis is simply one attempt at finding a way of more securely tying inferences about past cultural systems to the remains that they produced.

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IX

A Strategy for Perceiving Social Groups in Puebloan Sites

by

DAVID R. WILCOX

INTRODUCTION

A methodological problem of long-standing interest in puebloan archaeology is the perception of social groups in the context of archaeological phenomena. This interest is to be expected, since an important theoretical problem in all of Southwestern archaeology for nearly a century has been "How did modern Southwestern societies come to be as they are?" Prerequisite to a scientific solution to this master problem are scientifically warranted, non-trivial, and appropriate theoretical constructs of social groups, defined on the domain of archaeological evidence. A construct is scientifically warranted when measurable differences in specified empirical situations require measurable changes in the form of the construct and in how it is related to other constructs. A social group construct would be appropriate and non-trivial to the extent that, in the ethnographic domain (as will be discussed below), it facilitates explanation of classes of archaeological evidence, and, when translated into an ethnological universe of discourse, it is valuable for making cross-cultural comparisons. Strategies toward solution of this fundamental problem may be found at the heart of many classic papers and monographs in Southwestern archaeology.

Although the Bandeliers (1884) and Mindeleffs' (V. Mindeleff, 1891; C. Mindeleff, 1900) work takes precedence, it was T. Mitchell Prudden (1903, 1906, 1914, 1918) who in a series of problem-oriented papers clearly set forth a statement of a strategy for perceiving social groups which has been greatly elaborated and refined by subsequent workers. In brief, he suggested that the boundaries of architectural units may be used as indicators of social group boundaries. This solution reduced the methodological problem to one of establishing warranted procedures for identifying culturally meaningful architectural boundaries. Through

survey and excavation, Prudden demonstrated that pueblos in the northern San Juan watershed had one generic structure and that larger sites were composed of *units* of that structure. He (1914, p. 34) indicated that the "unit type pueblos" could be recognized as "marking family or clan units."

Prudden's approach was extended by J. Walter Fewkes (1919) who considered contiguous sets of Prudden's unit-type pueblos as indicating more complex social groups than the unit-types alone. Fewkes (1919, p. 16) argued that large (Pueblo III) "communal houses" were composed of sets of unit-type pueblos, and he (1919, pp. 70-71) used the presence of kivas as an index to the number of unit-type subsets. Prudden's work was also accepted by A. V. Kidder (1924) and it formed an important basis for both Strong's (1927) and Kroeber's (1928) syntheses. Steward (1937) later added Roberts' (1931, 1932) work to that of Prudden and Fewkes to come up with his own interpretation.

Wall abutment analysis was applied as early as 1907 by Kidder's colleague S. G. Morley (1908) in an effort to distinguish distinct architectural units and their construction sequence at the Cannonball Ruin. Years later, Lawrence Roys and Paul S. Martin (Roys, 1936, pp. 115-142, 194-209) worked out a method at the Lowry Ruin for analyzing building technique. They used that analysis in conjunction with "wall abutment analysis"—a term provided by Roys (1936, p. 135)—to synthesize a behaviorally meaningful building sequence. This was three years before Kluckhohn's (1939) call for typologies which were behaviorally meaningful. In 1929 and in the early 1930's, Frank H. H. Roberts (1931, 1932, 1939) in a series of sites combined wall abutment analysis and analysis of building technique and workmanship with that of stratigraphic relations in order to construct an argument—his (1931, p. 90) term was "story"—deducing a growth sequence of room-set additions and showing social change. That he did not stop there may be judged by examining his discussion of Unit No. 2 from the Whitewater district (Roberts, 1939, p. 196):

On the basis of comparison with practices among some of the modern village dwellers in the area the division in this structure suggests that the group here may have consisted of three *units* or families, a family consisting of husband, wife, and children. The relationship in general possibly was that of mother and father with unmarried children in one apartment and a married daughter with her husband and children in each of the other two. The likelihood of some such status is indicated by the evidence that two of the dwelling rooms, 9 and 11, as well as two of the storage rooms, 7 and 12, were subsequent additions to the original nucleus of 8 and 10 [a dwelling and storage room, respectively]. While it cannot be proved definitely that such was the order, foundation and floor levels, in relation to the old surface, indicate that 12 was added first, then 11, followed by 7, and finally by 9. [Emphasis added.]

On Alkali Ridge at Site 13, Brew (1946) combined analysis of building technique and workmanship with that of the spatial relations among room classes of different function to define the boundaries of different social groups (Brew, 1946, p. 193):

As has been shown above, the room walls presented a considerable range of variation in materials and methods of construction, of which only the most striking have been listed. One of the more interesting and instructive parts of the study of the site lies in the interpretation of significant variations within this range. In any quarter the wall construction varied regularly by groups of storerooms, in the following manner. The 2 or more storage rooms adjacent to a single living room often had walls built according to one style. The next group on either side was sometimes quite different. This apparently was a variation in individual technique or family fashion. Although wall construction in some sections was too uniform to permit the entire site to be divided in this way, certain general conclusions can be drawn. It seems that, while the rooms were all parts of a structure of contiguous elements, they were built and occupied in groups of 2 to 4 storerooms in association with and adjacent to 1 or 2 living rooms. This is identical with the custom in Hopi Pueblos today.

The work of Arthur Rohn (1965, 1971) and Jeffrey S. Dean (1969a, 1970) present recent versions of this traditional strategy, adding greatly to its scientific rigor and philosophical elegance. While using a variety of relations, Rohn has emphasized the relation of access among restricted spaces to define what I call *local aggregates* (Allen and Richardson, 1970, pp. 48-51) and sets of those. Local aggregates are sets of people who cooperatively perform sets of activities in distinct locations. Dean (1969a) demonstrated the great potential of dendrochronological approaches for distinguishing between what he calls absolute and classificatory contemporaneity of site structures. His analysis of tree-ring dates, in conjunction with other dimensions of archaeological evidence, laid a basis for his synthesis of what, again, I call absolutely contemporaneous local aggregates, sets of them, and intra-regional community dynamics.

Both Roberts (1939, p. 196) and Brew (1946, p. 193) and later Rohn (1965) and Dean (1969a) used more than culturally-meaningful architectural boundaries alone as indicators of social group boundaries. All emphasized the importance of sets of contiguous, functionally distinct room spaces whose boundaries matched those of the culturally-meaningful architectural boundaries. Rohn (1965) and Dean (1969a) each derived "socio-economic groups" by demonstrating the co-distribution of architectural units and domains of "domestic cooperation." Rohn (1965, p. 69) concluded, "I only wish to argue that archaeologists can delineate socio-economic units that approximate those described in ethnographic literature."

In light of this historical review, it is clear, I think, that recently published work conducted under the auspices of Field Museum of Natural

History by William A. Longacre (1970) and James N. Hill (1970a) also contains another version of this traditional strategy. Longacre (1970, p. 28) has summarized their approach:

Social demography and social organization are reflected in the material system. In a society practicing post-marital rules stressing matrilocality, social demography may be mirrored in the ceramic art of female potters; the smaller and more closely tied the social aggregate, the more details of design would be shared. Augmented by clues from other aspects of the cultural system *differential relative frequencies of designs may suggest the delimitation of various social aggregates*: larger social units such as the villages interacting in a relatively large area and producing pottery of the same Variety or Type; groups of villages forming a unit through social interaction along kin-based, religious, and political lines; the village as a social group; and residence groups forming a village. [Emphasis added.]

Bolstering their arguments with "clues from other aspects of the cultural system," they use the differential distribution of factors (derived from an application of factor analysis to sets of pottery design elements) to discern disjoint room sets which they take as indicating the boundaries of residence groups (Hill, 1970a, pp. 57-69; Longacre, 1970, pp. 38-40). That method for identifying residence groups is simply an alternative solution to implement the strategy enunciated by Prudden more than 65 years ago. The basic contribution they have made is to devote monographs to what earlier workers summed up in a sentence or a few paragraphs at most; Longacre and Hill have begun trying to do much more than that.

All the strategies designed to perceive social groups in prehistoric puebloan contexts together constitute a universe for methodological investigation. What is the logical structure of each method and ordered set of methods and to what extent is each logically sound? What are the relations in each method and method set between logical structure and the theoretical and epistemological aspects of it? Do the methods or method sets lead to mutually contradictory results or do some complement one another, both logically and semantically? If they complement one another, is this in total or only in aspects of each method set? Can a unified methodology be constructed which integrates complementary aspects from all the methods and provides all with a single, underlying logical structure? Is a single, underlying logical structure appropriate? Are different methods or method sets with different logics needed to define each social group construct? Questions such as these may be investigated in this universe of methods and strategies.

Strategies such as those reviewed above which elaborate and enrich Prudden's early approach constitute a subset of this universe. And as should be evident, successive researchers in that tradition have more or

less tended, although often implicitly, to present unified methods, integrating versions of valuable methods gained from their predecessors with innovations of their own. This is not to say that there are no other subsets in the universe of all strategies, though I am not aware of any others.

It is my purpose in this paper to contribute to investigations on this universe of strategies by carrying a step further the integrating process in the Prudden tradition subset. First, I will discuss an interpretation of what kind of logical structure is appropriate if useful social group constructs are to be invented. Then I will apply that interpretation to construct the outlines of a new, integrated strategy. This unified method then will be applied to analyze the building sequence and sequence of space usage at the Joint Site. My treatment of the unified method will be somewhat uneven, however, as the Joint Site materials provide an uneven opportunity to apply the various component methods of the strategy and it has not been made my responsibility to carry out analyses of artifacts or non-architectural features.

From an epistemological standpoint, my analysis of the Joint Site is of severely limited value. The conclusions which I offer, in my opinion, should be taken as alternative abductive inferences (see Peirce, 1958, pp. 89-164) in need of further testing before their status as confirmed facts can be evaluated. As of this writing (April 1972), I have not set foot on the Joint Site and, consequently, I have been wholly dependent upon the documentation made by the excavators and what more they could remember. Naturally, as in all situations of this sort, questions of fact raised by my analysis cannot always be satisfactorily resolved. In certain cases, dimensions of evidence which I would have considered important were not systematically investigated and can now be discussed only with qualification¹. Even so, granting these epistemological limitations, from a methodological standpoint, it should be possible to carry out a discussion of the logic of my analytic-synthetic approach to the site in terms of an understanding of what is currently the best estimate by the site's investigators of the true facts. This is then a preliminary analysis, and as such it is as much an opportunity to explore methodological and theoretical areas as it is an attempt to contribute to scientific knowledge. I will endeavor to mention the limitations of the estimates as the discussion proceeds. It is hoped that many of the empirical ambiguities can be

¹ It should be evident that the absence of particular sets of systematic information cannot be taken as criticism of the investigators. Such is the case with all field reports. It is hoped, however, that this paper will serve to bring out the potential value of several dimensions of archaeological evidence for discerning social groups in puebloan sites.

cleared up by additional field work in 1972 and that the hypotheses derived in this analysis can be tested and revised.

WHAT LOGIC IS APPROPRIATE?: AN INTERPRETATION

A primary assumption often applied in Southwestern archaeology has recently been stated explicitly by Thompson and Longacre (1966, p. 270):

This is that all of the material remains in an archaeological site are highly *patterned* or structured directly as a result of the ways in which the extinct society was organized and the *patterned* ways in which the people behaved. [Emphasis added.]

This statement I will take as fairly representative of the opinion of many Southwestern archaeologists. Because the concept of *pattern* in this assumption forms an underlying logical paradigm for methods in many versions of the strategies I wish to integrate, my discussion begins with a careful interpretation of how the logical import of this primary assumption should be understood. Four conclusions may be drawn:

1. As stated above, the assumption is somewhat ambiguous. Is it asserted that "all patterned human behavior and all organization of it results in a highly patterned archaeological record," or does only some patterned behavior or some organization so result? This question is trivial, however, since it is well established that there exists some organization of human behavior and some patterned ways of behaving which do not result in even slightly patterned material remains in archaeological sites. Non-trivial is the resulting problem of how to decide which kinds of organization and patterned ways of behaving do and which do not result in archaeological patterns.

2. It does not follow from the statement alone that all patterns discerned in the archaeological record are due to patterned human behavior or its organization. Sound methodology would require that rules be supplied for deciding which archeological patterns are due to the patterning of human behavior and its organization and which are not (Krause and Thorne, 1971).

3. A pattern is occasionally assumed to be indicated among several dimensions of data if they are "significantly non-random" in their joint sample distributions. Even if this were meaningful, as brought out in point 2, it would still remain to show that the "pattern" was not generated by non-human agencies. On the other hand, such assertions are incomplete. Samples are random only with respect to certain distributions and might not be with respect to others. What distribution is assumed? In

the case of finite populations, equal likelihood of selection must be demonstrated as a valid condition, not simply assumed. Perhaps it also should be recalled that a non-random phenomenon is either a chance phenomenon or a deterministic one (Parzen, 1960, p. 2).

4. How the term "patterned" in the statement above is to be interpreted logically is not clear. The difficulty is that "patterned" has at least two distinct meanings. Where that is the case, it is necessary to decide in each situation where the term is used, which meaning is appropriate. I would maintain, however, that where the problem is one of explaining socio-cultural change, that only one of the meanings is appropriate in any given situation.

Patterns are not defined on individual situations, single artifacts, or single behavioral events. Their domains are sets of similar situations, artifacts, or events. Differences in what is required to show similarity are the keys to differences in the meaning of the pattern concept. Pattern may be conceived either as a structural design of statically related classes or as the statistically regular outcome of "operations" on classes of propositions. In the former, similarity is established by demonstrating that a certain set of static relations exist in a series of situations, artifacts or events. Similarity in the latter depends on showing that the same actions, either behavioral, non-behavioral or both, recurred in a regular way to generate the pattern set.

An example illustrating these differences may be seen in the interpretation of "trash" in the post-floor deposition of room spaces. Suppose that in a class of room spaces sherds, vegetable refuse, lithic debitage, and broken grinding stones in relatively high frequency occur in association in a zone above the room floors but below roof debris and sterile fill. The terms "high frequency," "in association," "above," and "below" each specifies relations which are satisfied by the classes "sherds," "vegetable refuse," etc. Taken as a whole, these classes and relations form the parameters of a structural design which may be labeled "trash." Alternatively, one could ask, "how did these sherds, etc., come to be deposited here with these associations, etc.?" An hypothesis to answer this question could be that people on many occasions dumped quantities of household sweepings or other garbage into vacated room spaces. Such actions (which from a logical standpoint I treat as "operations") would result at first in a small pile or spread of material on the vacated floor surface, and eventually in a piling-up of such material. This hypothesis could be tested by further observations to see if the sherds, lithics, refuse, and grinding stones distributed within the zone in discrete clusters, to see

what the volume-shape of the zone was, or to determine if there was inter-bedding of sterile fill. If the hypothesis was confirmed in each room space, than the phenomena may be called "trash" as a summary way of indicating how it came to be there.

Time and processes are conceived differently in each pattern concept. A time interval of any finite duration may be conceived either *ex post facto* as an entity or while it is passing as instantaneous transitions through a series of moments. From an *ex post facto* perspective, a time interval is a static phenomenon; the flow of time has been reduced to a point, and so a behavioral event in that interval becomes an entity. From a dynamic time perspective, behavior is change occurring. What is of interest in the phrase "socio-cultural change" is then understood as changes of changes, or changes of changes of changes, etc. Processes from the former standpoint specify static relations among event-intervals, and these relations are derived by comparative observations of a collection of event intervals. The related concepts of diachronic and synchronic events are defined within an *ex post facto* time perspective (Greenberg, 1970, pp. 60-61).

Processes in the dynamic perspective are statements employing dynamic operations; that is, operations which have a time parameter as part of their definition. From this perspective, the statements called processes in the former specify quantum-jump transformations which never treat changes as phenomena in their own right (Greenberg, 1970, p. 62). Just as diachronic events are defined in terms of synchronic ones in the other perspective, here static characterizations can be defined in terms of dynamic ones. Structure is then the set of static relations which describes the outcome set generated by the dynamic operation statements.

Both of these pattern concepts have proved useful in archaeology. From a broader framework of the research goals, any contradictory conclusions generated from the two perspectives may be judged on their respective parsimony and the appropriateness of their logical basis. Concepts are like tools; there is no need to discard them if they are useful when used appropriately. Yet at the same time, where explanations of socio-cultural change are sought, and the focus is on the happening of the change, patterns are most appropriately conceived as outcome sets generated by dynamic operational statements. This is so because, from that perspective, time is viewed as happening, not as an unchanging interval.

Two significant advantages are gained if time is conceived as happening rather than as a static sequence of intervals. It opens the way for ex-

perimental work to further test explanatory statements. Archaeological evidence is all in the form of static relations among classes of presently existing phenomena. All statements about behavior patterns or changes of any sort must be inferential. From a logical standpoint, an inference which posits behavioral operations, if it is to be tested, should function logically to help explain sets of archaeological evidence. When such statements contain dynamic operations, specifying that under given conditions a certain action or sequence of actions were executed, if the conditions could be replicated and the actions carried out, then variations in the experimentally-derived outcome set could be compared with those in the archaeological pattern-set. If differences are noted, the archaeological phenomena could be re-examined and a more accurate or appropriate definition more in line with the experimental results might be drawn. Or, modifications in the conditions or operations could be made until a match is finally established. By this feedback process a better fit between archaeological fact and archaeological theory might well be established. Such an approach to lithic analysis has already proved its value (e.g., Jelinek, 1965; Crabtree, 1968; Jelinek *et al.*, 1971).

The second advantage concerns the difficult problem of identifying concepts in the archaeological domain which are on a high enough level of abstraction to be translated into culturally- or socially-meaningful concepts (Tuggle, 1970; Krause and Thorne, 1971). How can we go from arbitrary, static patterns to ones whose socio-cultural meaning is clear? Kluckhohn (1939) implied that what could be shown to be behaviorally meaningful was also culturally meaningful. That solution laid a basis for a whole tradition of typological research in American archaeology (Krieger, 1944, 1956; Spaulding, 1953, 1954, 1960; Jelinek, 1967, pp. 88-111) and it is a basic assumption in what I would now like to suggest.

When patterns are conceived as outcome sets, their meaning may be assessed in two ways. The set-inclusion rules for the set—that is, the conditions an element must satisfy to be a member of the set—posit meaning for elements in the set; and, the relations that the set itself has, as a class member, with other, comparable class members are also a source of meaning. The latter meaning is often deduced by applying such rules as the principle of superposition. The set of artifacts and refuse from post-floor context in room spaces labeled “trash,” for example, derives its behavioral meaning both from the hypothesis of how the elements in the set got there in the way they are, and from the relations the set stands in to room floors, roof debris, and sterile fill.

Set-inclusion rules here are not conceived as a set of attributes which an element must have to be a member of the set. Rather, they are defined as a set of propositions which together form a logical system (see Cohen and Nagel, 1962, pp. 129-147) and operations which act upon the propositions to generate a set. A proposition is the statement of a relation between classes (*Ibid.*, p. 123). In this manner, a hierarchical series of sets and their set-inclusion rules could be constructed, each lower-order set forming an element in a high-order one. A coherent system of meaning could thereby be established on a logical basis with only the lowest-order classes accepted as irreducible entities (cf. Moberg, 1971).

Logical systems on each level of analysis in such hierarchies would be logically independent of one another, though the meanings they impute would be quite closely related. Each proposition in a system, from an epistemological standpoint, would be an hypothesis which, sometime before the research activity is completed, could be tested. To the extent that axiomatic propositions could not be treated as hypotheses, theories based on them would not be parsimonious (cf. Hymes, 1964, p. 46). From a logical perspective, however, the truth of a proposition is not essential to its acting as a simplifying assumption or premise in the construction of arguments about which classes are and are not in a given set (Tuggle *et al.*, 1971, p. 4). For that reason, the implications of several propositions may be worked out before the statements are examined for their truth frequencies. (Cohen and Nagel, 1962, pp. 133, 169-172).

Since set-inclusion rules are conceived at some levels of analysis as logical axioms and operations, conclusions derived from them may be argued as theorems which are logically true and which, from an epistemological point of view, may be tested as hypotheses against appropriate dimensions of archaeological evidence. As both empirical statements and the logic of the arguments which connect them could be made explicit, field reports could be objectively judged as to their soundness and adequacy as theories accounting for the archaeological phenomena at a site. A theory conceived in this way (Kluckhohn, 1939) would be an interpretation of a logical calculus composed of component hypothetico-deductive systems (Cohen and Nagel, 1962, p. 133) but would not necessarily itself be such a system.¹ Statements about what people were doing at a site in specified situations may be construed as operations on various logical systems. The integrated combination of such systems and operations, together with all the confirmed theorems which could be deduced

¹ This concept of theory avoids the difficulties of the covering law approach to explanation discussed by Tuggle *et al.* (1971, pp. 4,5) without abandoning the hypothetico-deductive scheme altogether.

from them, may be taken as an ethnographic theory which describes aspects of human behavior that formerly existed at the site and, thereby, explains classes of archaeological evidence. If the behavioral statements are such that the assertions can be experimentally tested, the ethnographic language used to label the outcome sets and to specify the relational statements, operations, and theorems that give meaning to them could be translated from the ethnographic universe of discourse into a universe of many sites, the archaeological site and ethnographically-present societies, or both (cf. Goodenough, 1956).

From this point of view, field work may be conceived largely as a problem of perception, experimentation, and theory construction (cf. Krause and Thorne, 1971). A primary object would be to invent a language of sets, relations, and operations which could be used to model and to explain parsimoniously the field phenomena. To a great extent, a large archaeo-ethnographic lexicon of scientific concepts is already well established in Southwestern archaeology and it can be drawn upon as needed. With regard to some problems, however, fully adequate concepts have yet to be invented; indeed, problems of this sort help to define the frontiers of the discipline. They are a locus of archaeology as a pure science (Hanson, 1964, treats pure science in this way).

One such frontier problem is how to define "social groups" in puebloan sites. Within the context of the above interpretation of what kind of logical structure is appropriate, I will now offer the outlines of a solution to this problem and will then develop part of it in some detail in an analysis of the Joint Site.

THE STRATEGY

An archaeological site is a complex outcome of a series of interactions among human and non-human processes (Ascher, 1968). A site universe may be conceived then as partitioned into a temporally-ordered sequence of outcome sets, the last successively generated from earlier ones. In this way, the human occupation period may be viewed as a sequence of temporal intervals during which building or abandonment of archaeological features—such as room-sets, hearths, or mealing bins—took place, and periods of hiatus between building-abandonment episodes. Archaeological features co-existent during any given hiatus period would be what Dean (1969a, p. 198) calls *absolutely contemporaneous*. Absolutely contemporaneous physical features—including natural features with archaeological ones, together with their spacial relationships—form a domain or grid of structured space within which the behavior of people who used

those facilities may be plotted. Each domain of structured space I call a *site structure* (cf. Dean, 1970, p. 143). The concept of a site structure provides an operational definition of a "living surface" on puebloan sites. Significant changes in the living surface created by later building or abandonment activity, or natural processes, presumably are always correlated with changes in the way space was used in a site universe. The converse of this is probably not as valid. Changes in the use of space may occur without changes in the physical facilities being utilized. For this reason, inferences of social organization based on settlement pattern criteria alone are logically inadequate. If inferences of social organization are to be well grounded, it appears that they should be based both on inferences of how space was structured *and* of how it was used.

On the other hand, these two bases are not entirely independent of one another. Criteria of significance depend on both the problems being addressed and on considerations of how the space was being used. If the problems are ones of specifying changes in a group social structure, does a new opening knocked through a wall, mean one group, in expanding, has taken over space from another group, or does it mean simply that they wanted a new way to get into one of their rooms? The new wall opening indicates a change in the use of space, but does it also indicate a *significant* change, here one indicating a change in social structure? That can be determined only after it can be argued how the new wall opening is related to space usage before and after its occurrence. Thus it appears that the two bases are integrally related, like the two faces of one coin. Without the other face, the coin is counterfeit.

Methods for defining successive site structures and how each was used and modified, as well as a method for integrating these two syntheses are required in a strategy for getting at concepts of social groups, aspects of their social organization, and how they changed during the occupation period at a site. An approach to how space was used which satisfies these conditions may be derived by applying the three concepts of enactment, activity, and activity system in an analysis of the cultural materials. These concepts are defined as follows:

An *enactment* (Krause and Thorne, 1971) is simply any minimal behavioral operation. Examples: striking a blade off a core; laying a stone in a wall.

An *activity* is an ordered series of operations on a relational universe, transforming a set of propositions S by a sequence of n enactments into a new set of propositions. Examples: making a pot; building a wall.

Defined in this way, an activity is not simply an unordered set of material

classes but is an ordered set of propositions and operations. Posited in this way, the statements which specify an activity could function logically to account for sets of archaeological evidence. As the examples make clear, such accounts are available in earlier literature, often classified under the rubric "manufacturing techniques."

An *activity system* is an ordered set of activities and operations. Examples: building a room set; a daily domestic round. In addition, sets of activities and activity systems may be derived; for example, the full round of domestic activities.

To assert that a given set of activities is a system would involve specifying how the activities are related. Statements of these relationships should function logically to further account for aspects of the archaeological record otherwise unaccountable. In general, the objective boundaries between enactments, activities, and activity systems depend upon criteria of significance supplied largely by the problems which the research is directed toward solving.

From the standpoint of set theory, the spatial domain entailed and structured by specification of an enactment, activity, activity system or set of activities and activity systems can be interpreted as a universe of sets which in total is empirically identical to the universe on which the site structures are defined. By simply intersecting the sets of each domain, an integration of both may be achieved, yet their conceptual distinctness may be maintained.

This conclusion permits an operational definition of "social group":

The generic concept *social group* is any population set responsible for an activity, an activity system or a set of activity systems and activities for which the location in a site structure is known (cf. Freeman, 1968, p. 266).

This definition provides a broad opportunity to study a wide range of social relationships and how they changed among a wide variety of social groups. Differences among social groups may be distinguished by differences in the component enactments, activities, or activity systems in the sets, and by differences in the way these components are operated upon or related. The universe of all population sets defined in this way could by way of inference be meaningfully re-grouped into set concepts of task groups, socio-economic groups, households, etc. without losing any empirical accountability. Changes in site structure and enactment-activity-activity system definitions would require changes in these higher-order concepts and the relations among them. In this way the analytical study of socio-cultural change could be greatly facilitated and its scientific warrant largely improved.

SITE STRUCTURES AT THE JOINT SITE

Site structures in a site universe of physical features and their spatial relations are the grids of structured space which were exclusively co-existent during an interval of the site's existence. For purposes of the present analysis, this definition may be restricted to archaeological features alone. Changes in site structures then are brought about by either of two processes, viz., building or abandonment. Both processes occurred at the Joint Site, though there is no evidence that any spaces were re-occupied following a time hiatus after abandonment. By intersecting the building sequence with the abandonment sequence, the location and character of major hiatuses, and thus of site structures, may be displayed. The first question then is what classes of objects were built and abandoned? The answer depends upon the site at hand.

The Joint Site consists of two compact, single-storied, surface-masonry room blocks of 27 and five rooms, respectively, and five sub-surface structures. Two of the latter are spatially close to the large room block, one is below it, one is contiguous to the small room block, and the last is some distance southwest of both blocks. In such sites, two classes of phenomena are of fundamental importance in defining building sequence. They are "bounded occupation surfaces" (e.g., room floors, outdoor activity areas) and architectural units. The latter I classify into two disjoint classes. A *core structure* is all of the room spaces built as the original room block, and an *aggregation unit* is all of the contiguous room spaces added to a room block at any one time. The object of an analysis of building sequence is to isolate individual bounded occupation surfaces, core structures, and aggregation units and to show how each is related in time to the others.

Building sequence may be derived from the intersection of three broad dimensions of archaeological evidence, namely, stratification, from which hiatus may be inferred by applying the principle of superposition; distribution of building techniques, from which hiatus may be inferred from boundaries defined by transition to different techniques; and absolute dates, from which hiatus may be inferred from the way the dates cluster (Bannister, 1966, p. 124).

WALL ABUTMENT ANALYSIS

A first step toward the isolation of core structures and aggregation units may be made via wall abutment analysis (Roys, 1936, p. 135). An *abutment* is a relation between classes of stones, spalls, and mortar in two

walls such that at the intersection of the two walls none of these class elements in one wall overlap any of those in the other. Where any overlap does occur, the two walls are said to be related by a *bond* relation. Since walls have often partially fallen in—true for all walls at the Joint Site—the abutment relation must usually be inductively inferred from wall remnants. It also follows that if a bond relation holds for two wall remnants it holds for the whole walls; but if an abutment relation is true of the remnants it is not necessarily true of the whole walls. A further difficulty is introduced by the possibility of rebuilding two walls and bonding in a later addition (e.g., Martin, 1936, p. 38).

An existent abutment relation may then, but does not necessarily, indicate a temporal hiatus between the period of construction of each of the two walls. It is also possible that, due to the way construction proceeded, abutments were left between contemporaneous walls. Thus the possibility of hiatus at an abutment can only be evaluated in the context of other evidence. A *wall abutment analysis* is any set of procedures for evaluating the temporal significance of wall bond/abut relations.

The first strategy I will follow in executing a wall abutment analysis on the Joint Site evidence is to apply a logical system of two axioms to isolate sets of contiguous rooms which were built at the same time. I assume that 1) continuously bonded walls may have been constructed during a single building episode; 2) only fully enclosed spaces were being built. At an early stage of investigation these axioms could serve as simplifying assumptions which can be tested as hypotheses as excavation proceeds. The first axiom neglects rebuilding; at the Joint Site, however, no evidence of rebuilding wall-corner situations was recorded. The second axiom neglects the possibility that partially-walled-off court areas were being built.¹ This possibility would apply only in situations where abutments interrupted the extent of a wall around a space, as do a great many at the Joint Site. Yet no court areas are definitely evidenced at the site, either in standing wall remnants or from excavation results. In room spaces 2 and 5 there was evidence of a vertical post embedded in a wall (figs. 27, 28), and a post hole also occurred in both rooms. Possibly these posts were the supports for a roofed court area, but since the two post holes both were associated with plastered floors, this evidence alone is inconclusive. Thus, although the possibility still exists and is not limited to these two examples, in the absence of any definite evidence I will not consider this possibility further.

The second axiom also does not necessarily specify room spaces as

¹ I am indebted to J. J. Reid for this point.

they are presently delineated by standing walls.¹ If several room spaces are enclosed by a single bonded wall, and the "rooms" are formed by partition-like wall segments which divide up the space, then the "member" of the room set derived using these two axioms is the total enclosed space. The room spaces within that enclosed space may then be derived by a second application of the axioms.

To derive a room set using these axioms, a set-inclusion rule is needed: the minimal room set which satisfies these two axioms is called a *construction unit*. A corollary of the two axioms is that in any abutment situation the abutting wall is either later than or contemporaneous with the abutted wall; it is not earlier than the abutted wall.² When a decision as to which wall abuts the other cannot be made, both possibilities and their ramifications must be considered. From the two axioms and the corollary it follows as a theorem that: if all the walls of construction unit A which meet walls of construction unit B each abut to B, and none of A's walls abut to construction units later than B, then A is either later than or contemporaneous with B. A second theorem establishing a transitivity condition follows immediately: If construction unit A is later than or contemporaneous with construction unit B, and B is later than or contemporaneous with construction unit C, then A is later than or contemporaneous with C. Any sequence of construction units in which the transitivity condition holds throughout is called a *growth mode*.

These are important results as they imply that construction units are not necessarily either core structures or aggregation units; however, the latter are certainly composed of sets of one or more contiguous construction units. To see this, it is only necessary to notice that an aggregation unit is only later than the room set to which it abuts. While a construction unit consists of contiguous rooms built at one time, a core structure or aggregation unit is *all* the contiguous rooms built at the same time.

A methodological conclusion also follows. More evidence than wall bond/abut relations must be brought to bear if core structures and aggregation units are to be objectively identified. Nevertheless, limiting the

¹ The numbers in bounded spaces on Figure 29 I will treat as primarily labels of physical space. This, on the one hand, allows greater flexibility for discussion of pre-room occupation surfaces and the roof surface, and, on the other, it increases the precision of reference, since "room space 3" may be defined to mean "the space-time loci" when space 3 was used as a room.

² Michael B. Collins taught me this; he also taught much else, including the point about intersecting separate lines of evidence, which I apply in this paper from a set-theory standpoint.

possibilities for the location of construction-unit sets also limits the possible locations of aggregation units and core structures.

A strategy of this kind is not entirely new to Southwestern archaeology, nor to that of the Hay Hollow Valley. John Rinaldo (in Martin *et. al.*, 1964, p. 49) applied two assumptions to analyze wall relations at the Carter Ranch Site. His first assumption is the same as the first axiom stated here. To my knowledge, the first explicit statement of it was by Paul Martin (1936, p. 38), though it appears to be implicit in the work of many authors. Rinaldo's (in Martin *et. al.*, 1964, p. 49) second assumption is as follows:

(2) that walls which are built of neat appearing masonry . . . were at first exterior walls with neat-appearing face exposed, and that any abutment which hides the neat-appearing face ends a wall of later construction.

This assumption is not especially useful at the Joint Site, because few abutment situations like that indicated are found there.

A complete record of bond/abut wall relations was made at the Joint Site. Room corners were systematically excavated in 1971 to accomplish this and to facilitate a room map (fig. 6). Photographs were taken of each wall. Walls along the perimeter of the large room block and all of the small block were quite low, less than 50 cm. high. Those interior to the large room block often stood well over 1 m. high. The bond/abutment data are therefore somewhat uneven; several more bond relations may well have been true than are indicated in the perimeter areas. Three corrections in wall positions shown in the field map (fig. 6) were clearly indicated by photographs and the map in Figure 29 has been redrawn accordingly. The changes are in the south walls of spaces 2 and 5, and the east wall of space 20.

Rooms [28, 29] in the small room block form a core construction unit. All walls which meet it abut onto it. A single growth mode incorporates all of the rooms in the block; it may be expressed as follows, from earliest to latest: [28, 29] - [30] - [26] - [27]. If there are any hiatuses between building events in the small block, when room space 27 was built, all the other rooms were present.

The large room block is more complicated. The core construction unit is [13-17, 18], where the south wall of space 13 is possibly a later partition wall. Due to the large number of abutments, quite a few sequences of construction units could be written out. Yet from the intersection of all possibilities several general conclusions may be obtained. Consider first the west wall of room 1 (fig. 30). Depending on whether the abutment relation between this wall and the north wall of room 1 is considered first, or that between the wall and the north wall of room 2, two equally satis-

factory minimal room sets may be derived by applying the two axioms. These are [1] or [1, 3-4]. Since at present there is no logical way to choose between these two possibilities, both should be separately considered. The same conditions hold for the west wall of room space 7 and of space 21.

A second condition which necessitates consideration of additional possibilities is the multiple number of intersecting growth modes. Consider the possible sequence [13-17,18] - [20] - [19] - [16]. When room space 16 was completed, had room space 1 or room spaces 1 and 3-4 been built? Since there is no logical way at this stage of the analysis to answer this question, all three possibilities should be considered. If room space 1 is assumed not built at that time, then spaces 12-2, 3-4, and 1 would form the next construction unit in the growth mode possibility. With this much said, the rest of the possibilities may be readily worked out.

From the intersection of all growth mode possibilities, several general conclusions may be stated. Construction units [8], [24], [25], and [32] could have been added to the room block after any other room. The growth-mode segment [22] - [23] - [24] is not earlier than room spaces 13-17, 18, and 21, but it might be earlier than room space 20. Growth-mode segment [7,10] - [9-15] - [8] is not earlier than the room set [13-17,18,1,3-4,5,11,14]. It might be earlier than room space 6 or 31, or it might be contemporaneous with the latter. The growth-mode segment [6] - [32] is not earlier than room set [13-17,18,1,3-4]. Growth-mode segment [19] - [16] - [25] is not earlier than room set [13-17,18,20].

A second approach to wall abutment analysis treats abut/bond relations as two among a much wider set of wall relations.¹ I will consider only three other relations in this analysis. If one starts in Figure 29 with the east wall of room space 23 and proceeds to look northeastward along the east walls of spaces 21, 18, etc., it may be observed that the walls each stand in a relation of contiguity to those adjoining on the south and north. This relation is true elsewhere, but not everywhere. When it does not hold, the walls form an offset relation, as between the south walls of rooms 10 and 11. I interpret walls in an offset relation as being potentially less structurally sound than those in a contiguous relation. Any lateral stress by a wall in the latter case would distribute along the whole length of each wall, whereas in the former it would be concentrated against a wall perpendicular to it; this would tend to buckle the latter. The same result would tend to occur if a stress vector headed in the opposite direction (like breaking a stick across your knee). When

¹ A term paper by Alan Sullivan helped to clarify my thinking on this point.

faced with building a core structure or some of the rooms of some aggregation units, the builders could mutually compromise all size-shape requirements of the new room spaces in favor of greater structural soundness. When an aggregation unit was to be built onto the perimeter of an existing structure, however, the limitations imposed by the existing wall positions along the perimeter probably would more often require too great a compromise of size-shape preferences than in the former situation. Structural soundness would probably be compromised somewhat more often by offsetting new walls. Thus offset relations in some frequency probably indicate a temporal hiatus, and thus an aggregation unit boundary.

Aggregation units are abutted against the perimeters of earlier room blocks. Thus the set of all linear runs of abutments along possible room block perimeters should contain the boundaries between all aggregation units and earlier room blocks. Combining the offset relations with perimeter runs, several conditional facts may be stated. Room set [7,10,9-15, 8] could have been added as a unit, as evidenced by the row of abutments along its west side, the absence of offset relations among its member room spaces, and the offset relation of the south wall of space 10 with the south wall of space 11. Similarly, room sets [6,32], [2,12,16,19,25], and [21,22,23,24] could have been added as units. Room set [1,13-17, 18,19,20,21,22,23,24] could be a core structure with room set [3-4,5,11, 14] an aggregation unit onto it. Other possibilities are left for the reader to derive.

Plastering a wall is an activity, and if a wall abuts against a plastered wall, then clearly an activity of plastering the latter wall occurred between the times when the two walls were built. Thus, abutment against plastered walls may indicate a time hiatus between two building events, and, furthermore, it is quite likely that such abutment situations indicate significant time hiatuses.

No systematic studies of these relations have been carried out in the field at the Joint Site. Examination of the photographic record, however, does reveal 11 situations where they appear to obtain. These situations are as follows: (figs. 31,30,32,27): both ends of the east wall of room space 3 and the north wall of room space 17 abut plaster; and the east end, south, and north walls of room space 12; south end, west wall of room space 20; north end, west wall of room space 2; and the south and west ends, west and south walls, respectively, of room space 11 all abut plaster. All of these relations are hypothetical at this point and those in spaces 12 and 20 are the least supported.

These relations all should be given a careful field check, and a systematic search in all corner situations for such relations should be made, before much significance may be given them. For purposes of discussion here, however, I will accept the propositions as fairly reasonable interpretations. If so, several inferences may be deduced: the east wall of space 3 and the north wall of space 17, and possibly the north wall of space 12 and west wall of space 20, are later partitions of the spaces 3-4, 13-17, 2-12, and 19-20, respectively; the other relations and the two in space 12 all mark boundary points of aggregation units.

BUILDING TECHNIQUES

Walls at the Joint Site are for the most part made of unfaced, irregular sandstone slabs laid in abundant mortar. A field analysis (Tracz, 1971) of wall composition showed that the ratio of wall rock to mortar, as measured by surface area displayed in a 1 m.-wide column of exposed wall, varied from 87:100 to 26:100, but this total range of measured variation was present in one continuously bonded wall. Corners were often seen to have very little stone in them; this often made it difficult to identify bond/abutment relations. Some walls appeared to be largely composed in some sections of mortar and small, rounded pebbles, while other walls exhibited coursed masonry (fig. 27). Most walls were thickly covered with an adobe plaster (figs. 27,28,30,31,32), though several photographs demonstrate that this was not universally the case (for example, see the north face of space 20 shown in fig. 31).

Thus the presence of major variations in wall composition is fairly well established at the Joint Site, and several distribution studies have been made. As yet, however, no analysis of building activities, as defined in this paper, have been completed. The methodological approach to this kind of study worked out by Lawrence Roys (1936, pp. 115-142) at the Lowry Ruin could be applied here. Until such an analysis has been made, I feel that further discussion of these questions may best be suspended.

WALL OPENINGS

Twenty-nine definite and two questionable wall openings were recorded in the large room block, as were two definite openings in the small block. Eight roof openings in the large block and one in the small block were also evidenced, either directly or by inference. These data are tabulated in Table 10. This universe of wall and roof openings I have parti-



FIG. 27. Joint Site: Room space 2, facing south. Post mold in south wall; south wall abuts plaster, east end; closed regular doorway in south wall; coursed masonry, west wall; plaster on east and south walls, but not on closed doorway. Arrow (30 cm. long) points north; meter stick in background.

tioned into four major, preliminary classes. *Doorways* are fairly narrow, rectangular openings which extend through the center of a room wall to near the floor, where most have a low stoop (figs. 27, 30). *Portals* are squarish openings usually well above the floor and usually off-center in a room wall—an exception to this definition in the north wall of space 21 may actually be a cave-in (for portals, see figs. 31,32). *Vents* are small square holes, at or near the base of walls, except the one in room space 31, north wall, which was situated well up in the wall. Four of the vents have lintels. *Roof openings* are the fourth class, but I have little other information on them. The distribution of the first three classes is shown in Figure 29. In addition to these openings, space 34, a big kiva, had evidence of a smokehole in the roof and was connected to room space 15 by a tunnel. Judging by photographs, the portal shown in Figure 28 connecting rooms 5 and 11 apparently was a T-shaped opening that was later partly blocked to make a portal.

This classification is little more than the first step in a study of how the openings were used. Doorways, portals, and roof openings all probably served to permit the passage of people and their goods in and out of

room spaces. Portals may have been designed to impede the movement of scavengers such as mice. Vents quite likely were not used for the movement of goods or people (too small), but probably were used to conduct air into room spaces for better ventilation and combustion. A great deal more work is needed, however, before the behavioral meaning of these phenomena is understood at the Joint Site. For purposes of discussion here, however, I think this preliminary analysis is adequate enough.

Doorways, portals, and vents are further partitioned into *open* and *closed*, where the latter relation means the opening is blocked up with stone and mortar. As an hypothesis, I interpret an open doorway or portal as a dyadic relation between two spaces such that, if a floor in one was still in use, a floor in the other was; and, if one space was abandoned, the other was too. Abandonment of a room space may be defined as discontinuance in the use of the highest occupation surface bounded by the room walls. I now suggest a second hypothesis: when a room space which was connected to a second room space was abandoned, but the second was not, that the wall opening between them was closed. Some evidence for this could be plaster on one side only of a blocked opening, taken as indicating continued usage of the space on the plastered side. Plastering



FIG. 28. Joint Site: Room space 5, facing south. Post mold in west wall; possible T-shaped doorway in south wall converted to portal; plaster on all walls. Meter stick in background.

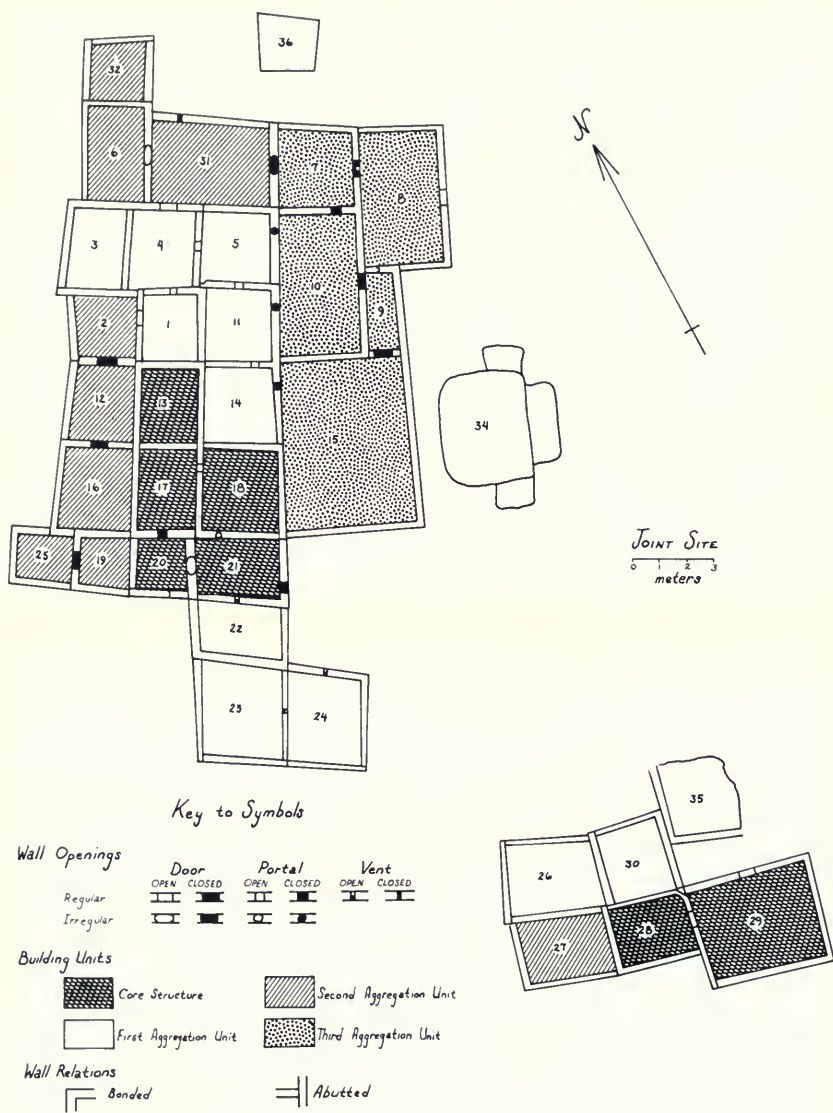


FIG. 29. Map of Joint Site Pueblo, showing wall relations, wall openings, and building sequence.



FIG. 30. Joint Site: Room space 1, facing west. West wall abuts plaster, south end; open regular doorway; plaster on all walls. Arrow (30 cm. long), points north; meter stick in background.

on both sides probably indicates continued use of both spaces. Judging by photographs, in these terms the dyadic pairs 9-15, 11-10, 9-10, 31-7, and possibly 14-15 were connected by blocked openings plastered on both sides. The pairs 17-20, 2-12, 7-8, 7-10, and 5-10 were connected by blocked openings plastered on only the latter side in each instance. Of the remaining three closed wall openings, the one in space 25 is not plastered, while the one in space 21 is, as is the presumed closed doorway in space 12, south wall. Thus room spaces 15, 10, and 12 continued in use after all wall openings connecting them with other spaces were closed and spaces 17, 2, 7, and 5 may have been abandoned at the time an opening was blocked up. The latter inference is not necessarily true, however, since the *absence* of plastering *per se* does not necessarily evidence non-use of an occupation surface in a room space. Once again, although more testing of these hypotheses is necessary, I will accept them here.

Wall openings are also partitioned into *regular* and *irregular* openings. The former may be profiled with straight lines, the latter only with wiggly ones. I infer that the former were built when the wall was, while the latter are openings knocked through solid walls. There is some evidence that special care was taken when openings were put in while the wall was

being built. Observations in the field (Tracz, 1970, pp. 12,13), presumably of regular openings, suggested that around wall openings harder and larger stones were used as compared with other areas of the wall. Photographs show that whereas thresholds or lintels do not always occur, when they do they are associated with regular openings. Knocking a hole through a masonry wall would usually produce an irregular opening, due to the inter-bedding of the wall stones. This naturally would vary with the amount of wall stone in the wall. Thus I conclude that irregular openings were put in some time after the wall was built. It is interesting then that all five vents are regular.

Wall openings in general mark a dyadic relation between two spaces. Openness of wall openings like doorways and portals has often been called *access* (Rohn, 1965). An irregular opening marks a change from non-access to access of some kind. Similarly, a closed opening marks a termination of an access relation. A closed, irregular opening evidences two changes, from non-access to access, and then back to non-access. It should be carefully observed that the relation "non-access" as used here does not mean it is not possible for a person to get from one space to an adjoining space; it does mean that movement of a certain kind is not permitted.

Openings built when the walls were built may connect either two room spaces or a room space and an outdoor space. Two doorways, one portal, and two vents which satisfy the latter relation may be seen in spaces 8, 20, 21, 24, and 31, respectively (fig. 29). I am aware of no reason to doubt that some others of the regular openings at one time also satisfied that relation. There is some likelihood, for example, that vents at the time of construction connected new room spaces to outdoor space, though the three vents still open and connecting two room spaces each at the time of site abandonment are evidence that this was not a necessary condition to their adequate functioning. Possibly, openings connecting room spaces and those connecting a room space to outdoor space at the time they were built were distinct from one another in specific methods of construction, but no systematic studies along this line have yet been carried out.

Therefore, although it cannot be concluded that a relation of an open, regular doorway or portal between two room spaces means the two rooms were built at the same time, it can be deduced that they were used contemporaneously for some time up until their abandonment. Rooms 17-18-21-20 and 2-1-4-31-5-11-14 are both connected in this way. Even if room space 31 was built later than room space 4, it was still used in

conjunction with the room space 4 and other spaces similarly connected to space 4.

Blocked, regular doorways or portals connect spaces 8-7-10-9-15, but only blocked, irregular ones connect that room set to the rest of the room block. I see this as more evidence that these five room spaces were built as one or more aggregation units to the earlier room block. The row of blocked, irregular openings also strongly indicates that room set [8,7,10,9,15] was at one time on an equal access footing with room set [2,1,4,31,5,11,14] and possibly [6,12,16]. In a similar way it could be argued that spaces 32, 22, 23, 24, and 3 were each isolated rooms which never shared these "access" relations with other room sets. Unfortunately this hypothesis cannot be tested because the wall remnants were so low in each case that the possibility of portals into adjoining rooms cannot be precluded. At one time, room spaces 17 and 18 were connected through room space 20 to outdoor space. Room space 12 and possibly 16 were connected at some time to room set [2,1,4,31,5,11,14] or to some subset of this. None of the spaces in the set [13,17,18,20,21,22,23,24] is known ever to have shared an "access" relation with other room spaces not in this set.



FIG. 31. Joint Site: Room spaces 17 and 20, facing north. North wall, room 17, abuts plaster, both ends; open regular portal in east wall, room 17; plaster on all wall faces except south face of north wall, room 20. Meter stick against north wall of Room 20.



FIG. 32. Joint Site: Room space 11, facing south. South and west walls abuts plaster, southwest corner; open regular portal, south wall; plaster on all walls. Arrow (30 cm. long) points north; meter stick in background.

One last comment on the wall opening between spaces 1 and 2 is in order (fig. 30). It is definitely open, but there is some question about whether it should be classified as regular or not. It has now largely deteriorated, but early photographs show it as a fairly regular rectangle, slightly larger and apparently curved at the top. While the weight of evidence appears to support a classification as "regular," I would suggest that it is not definitely established that the opening was first built when the wall was. This point is of some importance, as may be seen below.

BOUNDED OCCUPATION SURFACES

Most human behavior takes place on physical surfaces of various sorts. Modifications of these living surfaces are created by processes of building and deposition by both human and non-human agencies. All modifications due to human processes are evidence of human behavior. To the extent that the surfaces used by a society can be identified and temporally ordered, the structural relations among modification processes can be mapped and explanation can be objectively sought to account for them. Traditionally in archaeology (see Wheeler, 1956) these structural

relations and operations upon them have been called *provenience*. Often living surfaces are bounded, for example, by a wall, a river, or perhaps a convention. The latter is a behavioral boundary (Krause and Thorne, 1971) formed by the discontinuities of human interaction; the second is a condition of the physical universe, while the first is an outcome generated by human behavior and the way it is bounded. Unmarked conventional boundaries may still show up in the archaeological record: what is the edge of a site? (Clark, 1954).

The modifications present in a bounded living surface may easily be treated as a set of relational statements (propositions), in which the boundary concept provides a set-inclusion rule. Room walls are both modifications of a surface and the boundaries of later living surfaces, called floors. Everything on that floor or in it is then a member of a set defined by wall boundaries. If the room space is part of an aggregation unit of more than one room, all of the room floors in that unit form a set. Because walls are also markers of behavioral boundaries, it is likely that these sets are behaviorally meaningful. By analysis of the structural relationships among members of these sets, explanation may be sought which specifies that meaning and which, by doing so, parsimoniously accounts for the archaeological phenomena. In other words, a most fruitful locus for research on enactments, activities, and activity systems is in the site universe of provenience relations.

The matrix of provenience relations also contains a great deal of information about building and abandonment sequences. Table 10 tabulates for each room space the number of floor surfaces and pre-room occupation surfaces for which evidence is available at the Joint Site. No studies in the field were made of the continuity of pre-room surfaces and soil zones from one space to the next, and my reconstructions are thereby somewhat suspect. No spaces in the small room block were excavated below the upper-most floor surface. In the large block, 17 spaces were excavated in total or in part to sterile soil, and room spaces 8, 23, and kiva space 34 were not excavated below the uppermost floor surface. All room floors were clearly marked by a layer of adobe plaster. No portion of a pre-room occupation surface was evidenced below room floors in the two disjoint room sets [25,12,17,20,21,14] and [32,31,5]. Up to the time building modified these spaces into rooms, they formed two disjoint non-use areas—though, naturally, people could have walked around there without modifying the surface. This evidence tends to support an hypothesis that the small room block was not built significantly earlier than a core structure (at least) in the large block—because it seems likely that

some trash would have been deposited in one of these areas, particularly the southern one. On the other hand, one of the areas, at least, may have been sterile because it was beyond the peripheries of any outdoor activity areas.

Below the earliest floors of room spaces [3,2,1,11,7] a single occupation surface above sterile soil and below room floors was evidenced by several pits. Spaces 9 and 10, south end, and in a different way, space 15, south end, both had evidence of three pre-room occupation surfaces. The area between spaces 15 and 24 was also excavated to a distinctive outdoor-activity surface. In sum, these spaces form a continuous area where outdoor activities of various sorts were performed, and this fact supports a hypothesis that the rooms built on top of these surfaces are later than some of the rooms built on sterile soil. In conjunction with the construction unit-growth mode evidence, this supports a hypothesis that rooms in the set [25,17,20,21] are in the core structure. Room spaces 12 and 14 were built after or at the same time as room space 1.

Since there are three use-surfaces below room spaces 9,10,15—and probably 8—and only one below room set [7,11,1,2], it might be thought that the former rooms were built even later than the latter. However, the situation is complex. While a full discussion of it is beyond the scope of this paper, several hypotheses about the nature of the sequence may still be in order. A remarkable fact is that whereas the deepest surface below room space 9 is 40 cm. below floor surface in the northeast corner (and the surface dips to the southwest), the floor plaster of room 9 is at about the same height as that in room spaces 7, 8, 10, and 15, and it is only slightly (less than 5 cm.) higher than the earliest floors further west. All of the walls of room 9 rested on ca. 7 cm. of dirt fill inter-mixed with charcoal and ash. The culture-bearing zone below that was a clay material with pebbles, much different from the sterile soil seen below western rooms. Under 10 cm. and 20 cm., successively, of this clay material and pebbles there were two occupation surfaces. The upper one had an ash pit in a surface like the activity area between spaces 15 and 24. The lower one had two post molds and a large pit which extended into space 10. Is this lowest surface continuous with the top of sterile soil encountered beneath the western rooms? If so, it appears that rooms 8, 9, 10, and 15 lie above a former swale or hollow which during the occupation of the site was filled up by human action or erosion until it was finally leveled by a dirt-ash fill previous to construction of rooms 7, 10, 9, 15, and probably 8. Ground surface just east of space 34 is ca. 10-15 cm. above hard caliche. Although the north end of space 15 was not excavated below floor plaster, in the south end three surfaces separated by burned plaster (identical to

TABLE 10. Tabulation of room floor areas, and wall and roof openings

SPACE NO.	AREA m ²	WALL		OPENINGS		ROOF OPENINGS	FLOORS			PRE-ROOM			FILL
		NORTHEAST	SOUTHWEST	DOR	DCR		1	2	3	1	2	3	
1	4.9	POR	-	-	DOR	(X)	X	X	-	X	SS		Tr+R+S
2	5.5	-	DOR	DCR	-	-	X	-	-	X	SS		RS
3	6.1	-	-	-	-	X	X	-	-	X	SS		R+S
4	6.4	UNEXCAVATED											~S
5	6.8	-	PCI	POR	POR		X	X	X	-	SS		Tr+R+S
8	8.5	UNEXCAVATED											~S
7	11.1	-	VOR	DCR	DCR	-	X	-	-	-	SS		Tr+R+S
8	16.3	-	DOR	DOI	VOR	DCR	X	X	▷				R+A+S
9	3.9	DOI	-	DCR	DCR	-	X	-	-	X	X	X	RAS
10	14.7	PCR	DCR	-	2PCI	X	X	X	-	X	X	▷	RA+S
11	6.7	POR	PCI	POR	-	-	X	-	-	X	SS		Tr+R+S
12	6.7	DCR	-	DCR	-	(X)	X	X	-	-	SS		S+R+S
13	6.2	UNEXCAVATED											
14	7.2	POR	PCI	-	-	▷	X	-	-	-	SS		S
15	29.2	DCR	-	-	PCI	X	X	X	-	X	X	X	RAS+S
16	7.2	UNEXCAVATED											
17	6.5	-	POR	PCR	-	(X)	X	-	-	-	SS		Tr+RS
18	8.2	UNEXCAVATED											
19	3.8	UNEXCAVATED											
20	3.9	PCR	DOI	DOR	-	-	X	-	-	-	SS		S+RS
21	7.1	PCI	PCR	VOR	DOI	-	X	-	-	-	SS		Tr+R+S
22	6.7	UNEXCAVATED											
23	10.2	-	VOR	-	-	-	X	▷					RA+S
24	9.1	VOR	-	-	VOR	-	X	-	-	X	▷		▷A▷
25	3.3	-	DCR	-	-	-	X	-	-	-	SS		RS
31	13.2	VCR	DCI	DOR	DOI	-	X	X	-	-	SS		RA+S
32	4.4	-	-	-	-	-	X	-	-	-	SS	-	RS
34	10.7	-	-	-	-	-	X	▷					R+S
26	11.0	-	-	-	-	-	X	▷					R+S
27	7.1	-	-	-	-	-	X	▷					S A▷
28	7.2	-	DOR	-	-	-	X	▷					R+S
29	16.5	DOR	-	-	DOR	X	X	▷					R+S
30	6.8	-	-	-	-	-	X	▷					Tr+PS
35	6.5	-	-	-	-	-	X	▷					

KEY

P	PORTAL
D	DOORWAY
V	VENT
O	OPEN
C	CLOSED
R	REGULAR
I	IRREGULAR
-	NO EVIDENCE
X	PRESENT
SS	SUBSOIL
(X)	INFERRED
Tr	TRASH
R	ROOF CLAY
A	ROOF ARTIFACT
	ASSEMBLAGE
S	STERILE SAND
+	"IS BELOW"
▷	UNKNOWN
Tr+R	MIXED TRASH AND ROOF DEBRIS

room plaster) lay 15-25 cm. below floor level and dipped slightly to the north. These plaster layers were separated from the earliest floor plaster in space 15 by an 8 cm. layer of fairly homogenous clay followed by a thin zone of ash and burned clay on top of 7-10 cm. of tightly consolidated pebbles, clay, ash, and some artifactual material.

In general, it appears likely to me that the two profiles may be correlated. The early plaster layers I infer are correlated with the earliest

surface in space 9, the ash and burned clay zone go with the next higher surface in space 9, and the dirt and ash fill zone directly below the floor of room 9 is a localized phenomenon. Naturally, a more detailed analysis and additional field research on this problem is needed before too much confidence is given to any interpretation. I would suggest, however, that the available evidence does tend to support an hypothesis that rooms on the east side of the large room block were built later than most of those to the west. The evidence is inconclusive on the question of whether or not the eastern rooms were built as a unit or in sequence.

TREE-RING DATES

Charcoal and a few rotten-wood specimens were submitted to the Laboratory of Tree-Ring Research, Tucson, Arizona, for tree-ring analysis. After the specimens were studied by R. Warren and his results were checked by J. S. Dean, 47 dates were reported (letter from Dean to M. Schiffer, January, 1972). These dates and their provenience are tabulated in Table 11, and my discussion will proceed in terms of the table.

The interpretation of tree-ring dates has been elegantly discussed by many authors (e.g., Gladwin, 1945, pp. 119-152; Bannister, 1965, pp. 123-128; Dean, 1969a, pp. 10, 11; 1969b, pp. 29-32), and I will here attempt only to apply accepted principles to the Joint Site evidence.

Taking the dates as a whole (table 11), 39 are from roof context in kiva 34, five are from roof context in rooms 6, 10, 21, and 31, two were firewood in a late hearth in room 31, and one was associated with the earliest floor surface in room 9. There are three good clusters among the kiva-roof dates: 1247, 1244, and 1240. All 47 specimens were piñon, and since the growing season for piñon in this region is during the summer months, most of the specimens in each cluster were probably cut in the summer of that year. On architectural grounds, it is felt by the excavators to be extremely unlikely that kiva 34 was ever extensively repaired or re-roofed. If so, then probably the kiva was originally roofed in 1247 (most of the dates in that year are from primary beams) and wood from two earlier harvest events was either stockpiled or re-used later.

The dates from room contexts add some support to this contention. All dates before 1236 exhibit eroded outer rings, and cutting dates for all of these specimens could fall within a 1240 cluster. The earliest date, from room 6, is anomolous as well as isolated, and thus probably should not be accepted as either a cutting date or a construction date. Three dates, two from kiva 34 and one from a roof beam in room 21, form a weak cluster

TABLE 11. List of tree-ring dates from the Joint Site and their proveniences

SPACE	PROVENIENCE			SPECIMEN NUMBER	DATING	
	LEVEL	SECTION	ROOF BEAM OTHER		INSIDE	OUTSIDE
6	FILL		PRIMARY	ULC - 241	1020p	- 1188vv
9	SF 1	1	FLOOR CONTEXT	ULC - 242	1192p	- 1238r comp
10	A	2	PRIMARY	ULC - 122	1203p	- 1235vv
			PRIMARY	ULC - 123	1175p	- 1239r inc
21	C	3	UNKNOWN	ULC - 125	1159p	- 1222vv
31			FIREWOOD	ULC - 238	1201fp	- 1245vv
	B	3	UNKNOWN	ULC - 240	1204fp	- 1245vv
			FIREWOOD	ULC - 239	1223fp	1255vv

KEY

ULC	LABORATORY OF TREE-RING RESEARCH CATALOG NUMBERS
fp	FAR FROM PITH
p	PITH RING PRESENT
v	VARIABLE
vv	VERY VARIABLE
c	COMPLETE OUTER RING CIRCUMFERENCE
r	PARTIAL OUTER RING CIRCUMFERENCE COMPLETE ON SPECIMEN
B	BARK PRESENT
comp	OUTER GROWTH RING COMPLETE
inc	OUTER GROWTH RING INCOMPLETE
SF	SUB-FLOOR

34	A	2	UNKNOWN	ULC - 154	1196p	- 1223vv
	A	1	UNKNOWN	ULC - 185	1194fp	- 1229vv
			SECONDARY >	ULC - 193	1193p	- 1236r comp
	FILL		SECONDARY >	ULC - 178	1203fp	- 1236r comp
	B	2	PRIMARY	ULC - 145	1203p	- 1238r inc
	B	6	PRIMARY	ULC - 200	1180p	- 1239vv
	A	1	UNKNOWN	ULC - 184	1157p	- 1240r inc
	B	1	PRIMARY	ULC - 181	1159p	- 1240r inc
	B	2	PRIMARY	ULC - 217	1167p	- 1240r inc
	B	5	PRIMARY	ULC - 158	1172p	- 1240r inc
	B	3	SECONDARY >	ULC - 204	1184p	- 1240r inc
		6	SECONDARY	ULC - 189	1191p	- 1240r inc
	A	6	SUPPORT >	ULC - 220	1192p	- 1240r inc
		6	SECONDARY	ULC - 192	1191p	- 1240r inc
	B	5	UNKNOWN	ULC - 155	1198p	- 1240r inc
	B	5	PRIMARY	ULC - 153	1200p	- 1240r inc
		6	SECONDARY	ULC - 188	1207p	- 1240r inc
	B	2	SECONDARY >	ULC - 139	1166p	- 1240r inc
	B	1	PRIMARY	ULC - 183	1204p	- 1242vv
	A	1	UNKNOWN	ULC - 186	1200fp	- 1243vv
	FILL		SECONDARY	ULC - 177	1206fp	- 1244vv
	A	1	CLOSING MATERIAL	ULC - 187	1207fp	- 1244vv
	B	2	UNKNOWN	ULC - 160	1195fp	- 1244r inc
	B	2	SECONDARY	ULC - 140	1206	- 1244r inc
	B	5	UNKNOWN	ULC - 157	1095fp	- 1245r inc
	A	6	SUPPORT >	ULC - 201	1203p	- 1246r comp
	B	2	UNKNOWN	ULC - 147	1207fp	- 1246r inc
	B	1	SECONDARY	ULC - 195	1202p	- 1247v
	B	2	PRIMARY	ULC - 213	1192p	- 1247r inc
	B	1	UNKNOWN	ULC - 171	1199p	- 1247r inc
	B	1	PRIMARY	ULC - 182	1199fp	- 1247r inc
	B	6	UNKNOWN	ULC - 219	1203p	- 1247r inc
	B	6	PRIMARY	ULC - 206	1216p	- 1247r inc
	B	2	PRIMARY	ULC - 215	1194p	- 1247c inc
	TT 13		UNKNOWN	ULC - 223	1208fp	- 1237vv
	TT 13		UNKNOWN	ULC - 224	1215	- 1240vv
	TT 13		SECONDARY	ULC - 225	1131fp	- 1242vv
	TT 13		UNKNOWN	ULC - 222	1163p	- 1244v
	TT 13		SECONDARY	ULC - 226	1196fp	- 1244r inc

NOTES

1. THE SPECIES OF ALL SPECIMENS WAS PINON
2. SEE ROBINSON AND WARREN, 1971, FOR FULL EXPLANATION OF SYMBOLS

which adds some credence to an hypothesis that room 21 was built early. The two dates from roof beams in room 10 may with some confidence be interpreted as evidence for construction of the roof ca. 1239. This is 17 years later than the roof-beam date from room 21 and eight years before the kiva 34 construction date. The date from early floor context in room 9 indicates room usage by that date or later, and tends to support the room 10 interpretation. Dates on firewood in room 31 from a hearth in the upper floor indicate late usage of the room after 1255. Thus room 31 was being used after kiva 34 was built. The roof beam in that room is an isolated date and, although it may indicate primary roof construction after room 10 was built, it could just as easily indicate a repair job.

BUILDING SEQUENCE SYNTHESIS

By intersecting the various lines of evidence derived in the foregoing analyses of wall relations, wall openings, stratification, and tree-ring dates, a synthesis specifying the Joint Site architectural building sequence may be determined. Figure 29 presents what I see as the major outlines of the sequence, but within the sets illustrated and to some extent between those sets there are alternatives which cannot as yet be excluded as improbable.

Room set [13-17,18,20,21] is illustrated in Figure 29 as the core structure of the large room block. None of these rooms are known to have been built on an earlier occupation surface; together they form a set of contiguous construction units to which all adjacent walls abut. In all cases where photographic or other evidence is available (5 out of 10 cases, all on the north end), walls which abut this room set appear to abut against plaster. The earliest fairly-reasonable tree-ring date comes from roof context in room 21. A regular vent is in the south wall of room 21. With the exception of room 21, these spaces are all inter-related by regular portals and are not known to be related by any doorways or portal openings to other room spaces. There is evidence that the north wall of room 17 is a later partition wall.

Three fairly good alternatives are, first, that room space 19 was also part of the core structure; second, that room 21 was a slightly later aggregation unit; and, third, that only [13-17,18] is the core structure. There is some reason to believe that the west wall of 20 could be a partition wall. It is abutted on either end and one photograph suggests the wall abuts against plaster on the south end. The offset relation of the east wall of 21 with the south wall of 17 may lend some doubt to inclusion of room spaces 20 or 21 in a core structure with 13-17 and 18.

The best-defined room set, for which I do not see any equally probable alternatives, is the first northern aggregation unit, room set [1,3-4,5,11, 14]. Rooms 3, 1, and 11 were all built on top of an outdoor activity area. Together the rooms form a set of contiguous construction units, two of which abut against plaster along the perimeter of the core structure. Rooms along the southwest, east and north perimeter of this room set are related to it by offset wall-relations. These spaces are all inter-related by regular portals and are related to other room spaces to the east by irregular portals. There is evidence that the east wall of room 3 is a later partition wall.

An interesting regularity emerges from this analysis. All room spaces connected to another room space by a regular wall opening are related by a portal, not a doorway. All the regular doorways in the possibilities considered to this point connect a room space with what was outdoor space at the time the walls were built. If for some as yet to be explained reason these statements are *generally* true, the number of alternatives next discussed could be considerably reduced.

The second northern aggregation unit illustrated in Figure 29 is [32, 31,6], and the third is [8,7,10,9,15]. In the former, rooms 32 and 31 are built on sterile soil, while in the latter, rooms 9,10,15, and probably 8 were built above at least three earlier occupation surfaces, and room 7 was built above one. This stratification evidence is the principle reason for supposing that the latter room set post-dates the former, an assumption being that major new building is correlated with major modifications in outdoor activity surfaces. Each of these sets is composed of three contiguous construction units, but these each, in contiguous combinations, are possible aggregation units which satisfy all other criteria. No regular wall openings connect rooms in [32,6,31]. Regular wall openings do inter-connect rooms in [8,7,10,9,15], but two are doorways on a construction unit perimeter, and the two rooms in the unit are connected by a regular portal. If regular doorways at the time of their construction in most situations led from a new room space to outdoor space, then [7,10] - [9,15] - [8] would be an aggregation unit sequence, from early to late. Since all the presently enclosed spaces at the Joint Site are quadrilateral spaces (except room space 28), it seems unlikely that the wall between spaces 9 and 15 was ever a partition wall. The doorway in that wall segment may not be the anomaly it would at first appear to be, but may instead represent a special situation related to the kind of room 15 was.

A second aggregation unit to the southwest of the first northern one is shown in Figure 29 as [2,12,16,19,25]. These rooms are a set of contiguous construction units which abut against plaster along the perimeter

of the early room block; where there is evidence, these rooms are interconnected by regular doorways. There are many difficulties with this interpretation, however. Whereas room 2 was built on an earlier occupation surface, rooms 12 and 25 lie directly on sterile soil. The openings connecting these spaces are all doorways, and thus may indicate a succession of construction episodes rather than one alone. The presumed regular doorway between 2 and 1 would have to be interpreted as actually a late intrusion through that wall. An alternative that clears up much of this and is consistent with all other lines of argument is that each room, which here is also a construction unit, was also an aggregation unit. A complementary further alternative is that space 2 was a closed ramada area leading to an outdoor activity area further west. A third alternative which merits some attention is that the north wall of room space 12 may be a later partition wall.

The first aggregation unit to the south is shown in Figure 29 as [22, 23, 24]. Here, once again, there is as much reason to split these as to group them. Room space 24 may abut onto 23 and is connected to 23 by a regular vent. However, no offset relations are present and, because the walls are so low, the possibility of portals cannot be precluded.

The small room block I divide into a core structure and two aggregation units, principally on the basis of offset relations. Once again, the walls were too low to definitively argue that no portals were present, and until excavation is carried below the upper floor surfaces, little more can be said. It is interesting here, however, that a regular doorway connects room spaces 28 and 29 and did so at the time of construction. The interest lies in the similarity of space 29 to space 15 and the presence in both of a regular doorway leading to another room space built at the same time.

THE ABANDONMENT SEQUENCE

Modifications of a bounded living surface from one perspective means the creation of a new living surface; from another it is the abandonment of an old one. Five rooms at the Joint Site had evidence of several floor surfaces (see table 10). Room 12 had two definite plastered floors with different features associated with each. Plastered-over features or one floor feature intruded by another were recorded in rooms 1, 15, 31, and twice in room 5. Plastering over a feature or intruding another to build a new feature are modifications of a living surface. Thus a new assemblage-association is created, and the behavioral meaning of the later surface may be quite different from the preceding one, even though they share many of the same features. The event of plaster-

ing an upper surface in room 12 may correlate with that of blocking the two doorways in that room, but there is no direct evidence for or against this. If there were, it could be an indication of some kind of social-organizational change. The upper floor in room 15, on the other hand, evidenced by the addition of a pillar and relocation of a hearth, may be quite fortuitous, meaning no more than that the roof required further support. The east and south walls of room 15 were modified by a lot of rebuilding.

The fairly definite partition walls in spaces 13-17 and 3-4, and possibly the ones in 2-12 and 20-19, also mark abandonments of old floor surfaces by dividing them into two new ones. This kind of change could easily be a reflection of change in social organization. It is interesting in this regard to observe the size-shape similarity between the first three of these dyadic spaces and rooms 31, 10, and 8. The area sums and areas are 12.7, 12.5, 12.2, and 13.2, 14.7, 16.3 sq. m., respectively. The first three would be somewhat larger when the area filled by the partition wall is added. Together with room space 26, with an area of 11.0 sq. m. in the small room block, these six spaces are distinct from any others in either room block. The absence of a partition in the later rooms and their slightly larger size are of particular interest. A working hypothesis which accounts for these facts and which could be of some value in analyzing the features, artifacts, and refuse material is that the addition of a partition wall at this site marked a transition in a developmental cycle in domestic groups (see Goody, 1971) and that, later on, either the transition point was not reached again before abandonment of the site, or a different option was taken when it was reached. Two other facts which may have great bearing on this are 1) the association between room 15, a very large room space, and [10,8], and room 29, also a very large room space of similar proportions to room 15, and [26]; and 2) the absence of such relationship between the partitioned spaces and any other room space. Bounding a space like 15 or 29 may well indicate the working out of another social option.

The evidence of room fills as it bears on abandonment sequence must be discussed. When a roof falls in, anything on it falls in with the roof debris and is re-deposited in the same context or a slightly higher one, as the debris itself. The processes controlling natural roof deposition at a site are often regularly recurring phenomena, and it should often be possible to work out a general explanation which can be tested in individual rooms. Seeking an explanation for a roof collapse can be a useful strategy for determining what had been on that roof before it fell. Such an explanation would provide an objective basis for separating from the fill cultural

material formerly associated on a roof from that deposited via other processes—such as trash deposition. Once these different sets can be identified, the meaning of the provenience relations between them can be interpreted. If trash (a concept defined in the introduction to this paper as the outcome set due to throwing garbage into an abandoned space) occurs above all roofing debris and its associated artifacts in a room space, this relation between the class of roofing debris and that of trash means that the roof had fallen in or collapsed before the trash was deposited. If trash only occurs below all roofing debris, then it must have been deposited while the roof was still intact. From the distribution of different depositional histories of this sort, important inferences about site structure and the relative length of occupation can be deduced.

Fill in room spaces at the Joint Site, like that in many other Southwestern sites, was dug and collections were made in terms of arbitrary metric levels. Nevertheless, from the notes taken and from the nature of the collections some crude classifications which approximate what probably was the behaviorally-meaningful provenience structure may still be made with some assurance. These interpretations are presented in the FILL column of Table 10. Entries in that column for each room space state in propositional form the observed superposition relations between the classes trash, roof debris, roof artifact-assemblage, and sterile sand. The proposition for room space 11, for example, "Tr•RA•S," should be read from left to right as "Above the room floors was a layer of trash below a layer of roof debris and associated artifact assemblage and that was below a layer of sterile sand." This is a succinct description of the fill in room 11, and it can be quickly noted from Table 10 that the depositional history in rooms 7 and 21 was the same in these respects. The entry for room space 25, "RS," means that roofing debris without associated artifacts was mixed throughout the fill with sterile sand.

The classes used here were not formally defined in the field as outcome sets as discussed in the introduction to this paper (see pp. 120-125), but I will proceed to interpret them as though they had been. The partial mixing of collections due to the overlapping intersections of metric levels and natural zones does not affect the following argument as long as the gross set concepts and their superposition relations can be accepted. Natural fill at the Joint Site was wind-blown sand and it contrasted markedly with the clay consistently used to make the roofs. The roofing clay was also easily distinguished from the outcomes of trash deposition. Because these contrasts were empirically so sharply defined as gross constructs, I think there is more reason to accept the classes used here and the relations specified between them as approximations of appropri-

ate and adequate outcome sets than to reject them. Excavation of remaining room spaces could be used as an opportunity to re-evaluate these constructs to some extent.

An analysis of the propositional data in Table 10 yields several interesting conclusions and hypotheses. First, no trash was discarded in rooms where the roof had already collapsed (except apparently in room 5, where trash occurred above and below levels with roof debris), but it always occurred below sterile sand. In conjunction with the quarter-century span of tree-ring dates, this is evidence supporting a hypothesis that the site was not occupied for a great length of time. Second, although there is no direct evidence for it, a roof opening may be inferred for room spaces 17, 12, and 1. Trash in room space 17 was highest in the northwest quadrant of the room, well away from the open portal in the east wall. Such an accumulation could easily be created by throwing trash into a room space through a roof opening in the northwest quadrant. There was no entry in room 12 for two layers of sterile sand found below roofing debris except via a roof opening. Such strong evidence is lacking in the case of room space 1, since there are an open portal and an open doorway connecting that room space to two adjoining ones; but the possibility is worth mentioning.

Third, neither trash nor evidence of a roof artifact assemblage occurs in all room spaces. By taking "throwing trash into a room space" and "falling in of a roof artifact-assemblage" as set-inclusion rules, the sets "all rooms where trash was discarded" and "all roof space which was used as an occupation surface" can be formally generated.

The roof activity space shows up as a continuous area lying above the far north, east, and south room spaces: [31,7,8,10,11,9,15,21,23,24]. The trash depositional area also appears to be a continuous area, if undug rooms are neglected, and that area generally lies on the boundary of the roof activity surface: [7,5,11,1,17,21]. This evidence in conjunction with the building sequence possibilities I interpret to mean that room spaces [8,10,9,15] and [23,24, and probably 22] were in absolutely contemporaneous use at the end of the site occupation period. Room spaces 7,5,1,11, 17, and 21 were abandoned at that time and, since these rooms are connected by open portals or doorways to room spaces 31,6,4,2,14,18, and 20, it is likely that these were also abandoned then. Except for room spaces 31 and 7, these rooms were all in a core structure or first aggregation unit. The roof activity surface lies above room spaces in the northern and the southern growth modes. Because rooms in each were physically close to one another anyway, it is quite possible that there were actually

two bounded, roof occupation-surfaces, one above aggregation units in each growth mode; that is, [31,7,8,9,10,11,15] and [21,23,24, and probably 22]. To the extent that confidence can be placed in the classification of cultural material to a roof-assemblage provenience, the way roof occupation-surfaces were used may be studied.

This leaves roof spaces 32, 3, 12, 16, 19, 25, 13, and 22 yet to be accounted for. Four of these spaces are unexcavated: [16,19,13,22]. The sand below roof debris in room 12 may indicate that it was abandoned before the site was, but the same depositional picture could have obtained if the roof opening was left uncovered. Fill in the remaining three room spaces all had virtually the same depositional history, evidencing either a mixture of sterile sand and roof debris or the roof debris lying on the floor below the sand. Thus there is no reason in this evidence to doubt that [32,3,25] was fully in use right up to the time of site abandonment.

If that was the case, the absence of evidence that their roof spaces were used as part of an occupation surface places each of them in the same peripheral relation to the roof activity area on the east and south side and to the trash area between them and the activity areas. It is interesting that all three have been interpreted as storage facilities (Hanson and Schiffer, this volume, pp. 74, 60, 71). In sum, they are a row of storage facilities in use during the final period of site occupation by people living on the east side and south end of the room block.

Was the small room block occupied up to the end of site occupation? Room space 30 had trash in it and was undoubtedly abandoned before the site was. The rest of the rooms could have been occupied at the end of the site occupation, but the general absence of evidence for a roof activity assemblage in any room space (except possibly space 27) may cast this in some doubt.

SOCIAL GROUPS AT THE JOINT SITE

To conclude, I will summarize my findings in the form of several working hypotheses. It is hoped that they may be of some value to other investigators who will study the non-architectural features, artifacts, and flora and fauna.

1. The site was initially settled by three social groups which were distinct at a fairly high level of contrast, such as ceremonial-household groups. Two arrived on successive occasions, perhaps the third even later. The first group to arrive built the core structure in the large room block and the second built the first northern aggregation unit. The third group built the core structure in the small room block (perhaps all five rooms).

2. The rest of the site was generated by these three groups. The first group used the early pit structure (a kiva?) located below room spaces 9 and 10, and they also used the plastered surfaces located below room space 15, south end. The second group built and used kiva space 36 on first arrival. The first group expanded slightly to the south and west, while the second expanded greatly to the north and east, becoming the largest population set at the site. Kiva space 34 was probably built from stock-piled wood and newly-cut timbers in 1247, at a time when the second group was at its peak population. Probably room spaces 9 and 15 also were built by the second group at the same time or in 1244. The first group built space 33 (a kiva?), sometime during its expansion period. The third group built the small room block and kiva space 35.

3. The two groups that built in the large room block were initially organized in quite a similar way. The first group continued to have that structure, but the second one changed, adopting an organization like that of the third group.

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FIG. 33. Snowflake Black-on-White, Carterville variety, pitcher (cat. no. 263954); Joint Site Pueblo, found with burial no. 1. Greatest diameter, 12.5 cm.; height, 11.5 cm.



FIG. 34. Snowflake Black-on-White, Snowflake variety, bowl (cat. no. 263956); Joint Site Pueblo, found with burial number 3. Diameter, 26.5 cm.; height, 16.0 cm.



FIG. 35. Snowflake Black-on-White, Snowflake variety, pitcher (cat. no. 263957); Joint Site Pueblo, found with burial number 3. Greatest diameter, 11.5 cm.; height, 9.5 cm.



FIG. 36. Snowflake Black-on-White, Snowflake variety, bowl (cat. no. 263958); Joint Site Pueblo, found with burial number 3. Length, 29.5 cm.



FIG. 37. Snowflake Black-on-White, Snowflake variety, bowl (cat. no. 263960); Joint Site Pueblo, found with burial number 6. Diameter, 21.5 cm.; height, 9.5 cm.



FIG. 38. Show Low Black-on-Red, bowl, (cat. no. 263963); Joint Site Pueblo, found with burial number 7. Diameter, 17.5 cm., height, 10.0 cm.



FIG. 39. St. John's Polychrome, bowl (cat. no. 263965); Joint Site Pueblo, found with burial number 9. Diameter, 19.0 cm.; height, 8.5 cm.



FIG. 40. St. John's Polychrome, bowl, (cat. no. 263966); Joint Site Pueblo, found with burial number 9. Diameter, 29.0 cm.; height, 12.5 cm.



FIG. 41. Snowflake, Black-on-White, Snowflake variety, bowl (cat. no. 263970); Joint Site Pueblo, found with burial number 13. Diameter, 14.5 cm.; height, 7.5 cm.



FIG. 42. Snowflake, Black-on-White, Snowflake variety, jar (cat. no. 263974); Joint Site Pueblo, found with burial number 14. Greatest diameter, 18.0 cm.; height 14.0 cm.



FIG. 43. Snowflake Black-on-White, Snowflake variety, duck effigy (cat. no. 263981); Joint Site Pueblo, found on floor, room 7. Greatest length, 17.5 cm.; height at center, 9.0 cm.

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